PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXIX.

PART I.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, BOOKSELLERS TO HIS MAJESTY,
AND PRINTERS TO THE ROYAL SOCIETY.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

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be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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PHILOSOPHICAL

TRANSACTIONS

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on Sir GODFREY COPLEY's Donation, for the year 1818, to ROBERT SEPPINGS, Esq. for his Papers on the Construction of Ships of War, printed in the Philosophical Transactions; and the Gold and Silver Medals on the Donation of BENJAMIN COUNT of RUMFORD, to DAVID BREWSTER, LL. D. F. R. S. for his Discoveries relating to the Polarisation of Light.

PHILOSOPHICAL TRANSACTIONS.

I. *The Croonian Lecture. On the conversion of Pus into granulations or new Flesh.* By Sir Everard Home, *Bart. V. P. R. S.*

Read November 5, 1818.

As this Lecture was instituted with a view to discover the principle upon which muscular motion depends, which cannot well be done till we have acquired a more correct knowledge of the structure and mode of formation of muscles; I considered that I was not greatly deviating from the direct path of this enquiry, in making the changes extravasated blood undergoes, preparatory to its being incorporated with the muscular and other structures of a living body, the subject of my last year's Lecture.

The present Lecture may be considered as a continuation of the same subject, since it is my intention to examine the changes pus undergoes in the formation of granulations or new flesh; which will be found to correspond with those that were stated to take place in the blood, so nearly, that the two fluids will be admitted to possess the same properties; and that the red colour of the globules is the principal

characteristic mark of distinction between the one and the other.

As pus, in its first formation, has the appearance of being a transparent fluid in which globules afterwards are formed, whether the transparent fluid remains on the sore, or is removed to any other surface, as was proved by experiments made in the year 1788, and since that time laid before the public in my work upon Ulcers; in this respect, pus might be considered to differ from blood; but the following observations, made by Mr. BAUER, tend to prove that a similar formation of globules is met with in the serum of blood. I shall give the remarks in his own words: "That the globules in the blood are produced in the serum, I first observed in July 1817, when I examined a small portion of human blood on a glass plate, to ascertain the real shape and size of the globules. I then found in one square of the micrometer (which was the 160,000 part of a square inch) two of these globules which were separated to a considerable distance from the rest; they were entirely disengaged from the colouring substance, and lay in pure clear serum, which covered the whole surface of the square of the micrometer. Having placed this particular square immediately under the focus of the microscope, I attentively examined the globules for about six or eight minutes, when I perceived two extremely minute opaque spots arising in the clear serum within the same square of the micrometer, and which seemed increasing in size. In a few minutes longer, I perceived five or six more such opaque spots arising, and gradually increasing, and assuming the same form and appearance as the two original globules; but the moisture of the serum

being nearly evaporated, I diluted it with water, when all the seven new globules, as well as the two original ones, floated in the water, and appeared of precisely the same shape and white colour; and three of the new globules were of the same size as the original ones, but the rest were smaller. When left on the glass to dry, the globules remained of the same shape and size as they were whilst floating in the serum.

"The above experiment I have repeated a great many times with human blood, as well as with sheeps' and calves' blood, and the results have been always the same. When warm and fresh blood was used, the serum covering the surface of a 160,000 part of a square inch, produced from six to twelve globules, but when the serum was diluted with water, the number of globules produced was less, and they were smaller in size.

"On the 11th of August, 1817, I poured half a pint of warm sheep's blood into a glass vessel, and left it forty-eight hours at rest to coagulate: I then poured off the serum into another vessel, in which it remained at rest six hours; with this serum, a glass tube four inches long and three-eights in diameter inside was filled to overflowing, and closed with a good cork, and covered with a bladder. The serum was as clear as water; and although I examined it very attentively, I could not see more than fifteen or twenty globules in the whole extent of the tube. It was kept inverted in a glass of water. At the end of seven days, upon holding the tube between my fingers, which were tolerably warm, and examining it with a double lens of considerable magnifying power, I

saw some hundreds of globules rise from the bottom, and ascend in a straight line in the centre of the tube, and when arrived within about half an inch of the upper end, they spread in all directions, and descended close to the sides of the tube; when near the bottom they re-ascended, but more rapidly than the first time, and when held longer in the warm hand, the rapidity of the motion was much increased. In two days more, I found upon examination the number of globules much greater; and on the 25th of September, 1818, the number of the globules was such as to form a sediment at the bottom of the tube of half an inch in thickness, besides a strong coat on the inside of the tube."

This experiment of Mr. Bauer's on the serum, was repeated by Mr. Faraday, at the Royal Institution, on human blood, in a tube of larger dimensions, and the serum suspended on mercury: the result was exactly the same, the number of globules was increased in ten days in the same proportion as in Mr. Bauer's experiment, and when the lower end of the tube was held in a warm hand, the same motion of the globules took place.

These experiments in proof of colourless globules forming in the serum, make this resemblance between blood and pus greater than has been generally believed.

At the conclusion of my former Lecture, I mentioned that pus, in its inspissation, has carbonic acid gas evolved, in the same manner as in the coagulation of the blood, and that I was therefore led to the opinion, that this process was the first step towards the formation of granulations; but my experiments having been made upon pus removed from the

living body, they required being repeated upon the surfaces of sores, before their results could establish the opinion I had adopted.

Before I attempted to trace the changes met with in pus upon the surface of a sore, my first object was to become more accurately acquainted with the appearance of the surface immediately under the newly secreted pus. That this surface might be examined under the most favourable circumstances for such observation, I selected an ulcer upon the leg to which no application was made but straps of adhesive plaster, and these only changed once in the twenty-four hours; and that time was chosen for the examination, which was made by a double convex lens, magnifying about eight times. Previous to the adhesive straps being taken off, the leg was laid upon a low table, so as to be immediately under the eye, and in the position in which hæmorrhage from the small vessels was least likely to take place, and obscure the surface of the sore.

That the observations might be made with greater accuracy, I requested Mr. John Griffith, one of the pupils of St. George's Hospital, to look at the sores, as well as myself, upon every occasion on which they were examined, and no change is mentioned to have taken place that was not distinctly seen by us both. A healthy sore thus examined had the following appearance: the surface was uneven, being made up of eminences and hollows. The eminences consisted of small clusters of tortuous blood vessels, the hollows were filled with pus. After remaining exposed from five to ten minutes, the following alterations were distinctly seen to take place: a very thin pellicle covered the whole surface;

this was of so transparent a nature, that a number of small bubbles of gas were seen to make their appearance in different places; in a few minutes more, horizontal canals of different sizes, filled with red blood, taking different directions, and anastomosing with one another, were seen to form. In some places, there were red points, the terminations of perpendicular canals, that had been stopped in their course, by coming against the pellicle. There were also occasional specks of extravasation, from some of the horizontal canals bursting through the pellicle.*

The changes just mentioned seemed to occur in a regular order of succession. First, the pellicle was formed on the surface. Secondly, the bubbles of gas made their appearance. Thirdly, the canals carrying red blood were observed: these, while filled with carbonic acid gas, were not to be distinguished from the semi-transparent jelly which surrounded them.

As it is difficult to describe appearances of this kind, and it is of importance that the fact of such appearances being met with, should be well established, I requested Mr. BAUER to make a drawing of a portion of the sore of which I have attempted a description, after it had been exposed for nearly sixteen minutes; and on the following day he made a drawing of the same portion, showing the progress that had been made, and that the canals formed on the first day, had on the second become permanent tubes, and had been covered over by a cuticle. These two drawings are annexed.

* If, under these circumstances, the foot was put to the ground, so weak was the covering of the canals, that it instantly gave way, and the sore was covered with blood.

It is so easy for any one to bring the facts which I have stated under his own observation, that I shall leave them to speak for themselves, but it may appear to my audience, that farther evidence is required to establish the doctrine, that they are produced entirely by the coagulation of the pus, and the extrication of the carbonic acid gas. To remove every objection which it occurred to me could be made, I put the doctrine to the test of the following experiments. Immediately after the exposure of the surface of the sore, I poured water at the temperature of 95° upon it, which washed away all the pus and although the sore was left exposed in this state ten minutes longer, none of the above mentioned appearances were produced, so that the presence of pus is necessary to their taking place.

As cold water has a power of coagulating pus more rapidly than simply exposure to the atmosphere, I applied water at the temperature of 65°, to a sore, and all the appearances were produced in so much greater a degree, that I requested Mr. BAUER would make a drawing of a portion of a sore that had been exposed for fifteen minutes under common circumstances, and at the end of that period to pour upon it water at the temperature of 65°, and, in ten minutes more, begin a second drawing of the same surface, showing to how much greater an extent the appearances had taken place; by this means proving, that the degree of coagulation was the great cause of the effects that followed. These drawings are also annexed.

As a saturated solution of sal ammoniac has a greater power of coagulating pus than any substance that I am acquainted with, (and on that account, in the year 1788, I

recommended its mixture with pus as the best criterion by which pus might be detected and distinguished from other animal fluids,) I now determined to try what effect it would have with respect to the appearance of the granulations; for although in some respects it is not a fair trial, since the chemical combination of pus with this solution might destroy the natural properties of pus, and convert it into a compound of a very different kind, still that was by no means necessarily the case.

Upon pouring a saturated solution of sal ammoniac at the temperature of 45° upon the surface of a sore, the pus almost immediately became curdled, and tortuous canals were every where seen in these masses of coagulum. There was great uniformity in the tortuous canals; they were of the same size, running first in a straight direction, terminating in a spiral turn and a half, the end of which was extremely small; they were all filled with red blood. It was remarked that, although the canals themselves were in greater number, there were fewer bubbles of gas than when the cold water had been used, more having been retained in the tubes. Some of the coagula of pus were more elevated than the general surface, and large canals filled with red blood were seen superficially passing over some of them, without any smaller ones in the immediate neighbourhood. To ascertain whether there was any vascular basis with which these canals were connected, I passed a tolerably large crooked needle under one of them, bringing out the point on the opposite side, so that the canal was distinctly seen above the flat surface of the needle: I then withdrew it, and there was not the slightest degree of extravasation of blood. This was repeated on

several different sores without any appearance of blood escaping, or the person having the slightest pain; affording a sufficient proof of the canals being formed in the coagulated pus immediately on its coagulation, before any other approximation to living animal solids had taken place.

The readiness with which the blood displaces the carbonic acid gas contained in these canals, may be explained by the great disposition the blood has to absorb this particular gas, which forms so large a proportion of its component parts.

I shall not take up the time of the Society with a farther detail of experiments, although many more were made, as the results were uniformly the same.

If I have succeeded in establishing the object of this Lecture, which is, that the coagulated pus is rendered tubular by the extrication of its carbonic acid gas, and that these tubes or canals are immediately filled with red blood, and thus connected with the general circulation, there will be little difficulty in making out the succeeding changes, by means of which the coagulated pus afterwards becomes organized; since Mr. Bauer's drawings, laid before the Society last year, trace the thin covering of the canals in the coagulated blood to the thick arterial coats met with in the testicle after the coagulum had remained a month in that situation; and it is the arteries which build up all the different structures in the body, as well in the restoration of parts, as in their original formation.

The farther prosecution of this enquiry belongs to the science of Surgery; but as the explanation which I have given of the process employed in the regeneration of parts is, I

believe, entirely different from that which is generally received, I have been desirous that an account of that process should, in the first instance, be laid before the Royal Society.

DESCRIPTION OF PLATE I.

Two views of a small portion of a superficial sore on the leg, close to its edge, magnified 10 diameters.

Fig. 1. The appearance the surface put on after it had been exposed by the removal of the dressings for 10 minutes, none of the parts represented having been visible at the time the sore was first exposed, as it was covered with a thin cone of pus. The appearances since produced are the canals carrying red blood; the red points, which are terminations of perpendicular canals; and the bubbles of carbonic acid gas. The greater part of the margin is covered with a film of inspissated pus which is become cuticle.

Fig. 2. The appearance the same surface put on the subsequent day at the same hour, after exposure for the same time; showing the progress of the healing process, particularly the rapidity with which the sore is covered by cuticle.

DESCRIPTION OF PLATE II.

Tw views of a small portion of a superficial sore on the leg, magnified 10 diameters.

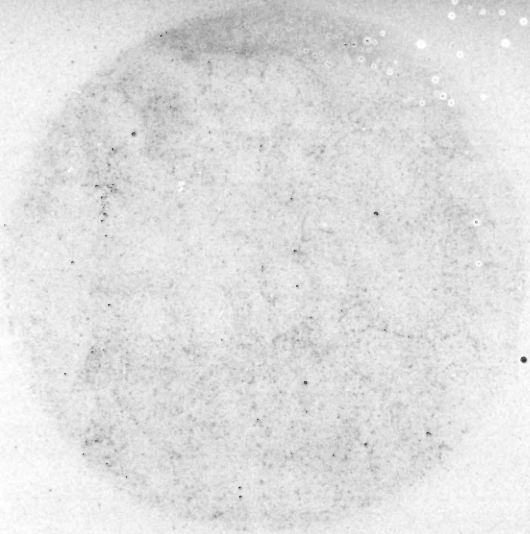
Fig. 1. The surface represented under exactly the same circumstances as in Pl. I. Fig. 1.

Fig. 2. The same surface represented 10 minutes after cold spring water had been poured over it for a few minutes, to show the increase in the number of canals.

Fig. 1.

Fig. 2.

Franc. Bauer del. Jos. Basire sc.



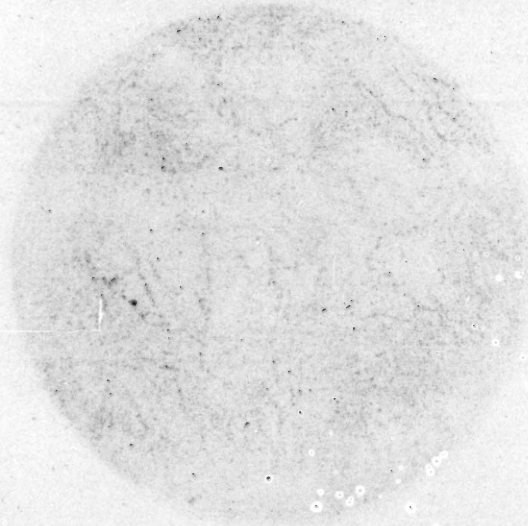


Fig. 1.

Fig. 2.

Franz Bauer. del.

J. Basire. sc.

II. *On the Laws which regulate the Absorption of polarised light by Doubly Refracting Crystals.* By David Brewster, *LL. D. F. R. S. Lond. and Edin. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G. C. B.*

Read November 12, 1818.

My Dear Sir,

In examining the polarising structure of acetate of copper, my attention was drawn to certain changes of colour which this crystal exhibited when exposed in different positions to polarised light. As this variation of colour was independent of the thickness of the plate, and of any analysis of the transmitted pencil, I had no hesitation in regarding it as a new affection of light, and in ascribing the phenomenon to the absorption of the homogeneous tints which formed the compound colour of the crystal. I therefore collected all the natural and artificial crystals which were characterized by any peculiarity of colour, and examined the various phenomena which they presented, when cut at different angles with the axis, and when exposed in different positions to a polarised ray. The results which I obtained during this investigation were singular and unexpected, and I am persuaded will throw considerable light on that property of transparent bodies, by which they detain and assimilate to their own substance a portion of the rays which penetrate them, while the rest are freely and copiously transmitted. As this faculty of absorbing light is related to the axes of double refraction, I

shall first describe the phenomena which are presented by crystals with One axis, and then explain the modifications which these phenomena undergo when the number of axes is increased.

SECT. I. *On the absorption of polarised light by Crystals with One axis of double refraction.*

If we fasten upon one side of a rhomboid of *colourless calcareous spar*, a circular aperture of such a magnitude that the two images of it appear distinctly separated when viewed through the spar, we shall find, by exposing it perpendicularly to common light, that the two images are perfectly colourless, and of the same intensity in every position of the rhomboid. Hence if Q be the quantity of transmitted light, we shall have the ordinary image $O = \frac{1}{2}Q$, and the extraordinary image $E = \frac{1}{2}Q$.

When the rhomboid is exposed to polarised light, the intensities of the images vary with the azimuthal angle (a) which the axis of the rhomboid forms with the plane of primitive polarisation, and may be represented by the formulæ $O = Q\cos.^2 a$; $E = Q\sin.^2 a$. But since $Q\cos.^2 a + Q\sin.^2 a = Q$ we have $O + E = Q$; that is, the sum of the intensities of the two pencils is in every position equal to the whole transmitted light, and therefore the rays which leave any one of the images by a change of azimuth, are neither reflected nor absorbed, but *pass over into the other image*. The ordinary phenomena of double refraction, consequently, afford us no reason for conjecturing that the crystals which possess this property absorb the incident light in any other way than is done by all other bodies, whether solid or fluid.

If we now take a rhomboid of certain specimens of *yellow calcareous spar*, and perform with it the experiments which have just been described, we shall obtain a series of entirely different results. The two images will now be found to differ both in colour and intensity, the extraordinary image having an orange yellow hue, while the colour of the ordinary image is a yellowish white. This difference of colour is distinctly related to the axis of the crystal, and increases with the inclination of the refracted ray to the short diagonal of the rhomb. It is a maximum in the equator, while along the axis the two images have exactly the same colour and intensity. In every position, however, the combined tints of the two images are exactly the same as the natural tint of the mineral. In comparing the intensities of the two images, the extraordinary one appears always the faintest, so that there is an interchange of rays; and while the extraordinary force carries off several of the yellow rays from the ordinary image O, the ordinary force at the same time takes to itself several of the white rays from the extraordinary image E; for if this were not the case, the extraordinary image would always have the greatest intensity, whereas, in consequence of its exchanging yellow for white light, it becomes actually fainter than the ordinary image.

If we call m and n the maximum number of rays which the extraordinary and the ordinary image interchange, and (φ) the inclination of the refracted ray to the axis, the intensities may be represented by the following formulæ when the crystal is exposed to common light. $O = \frac{1}{2} Q + \sin.^2 \varphi\, m - \sin.^2 \varphi\, n$ and $E = \frac{1}{2} Q + \sin.^2 \varphi\, n - \sin.^2 \varphi\, m$. The values of m and n vary in different crystals: they are always of different

colours, and in some cases they are equal to nearly one half of the transmitted light.

When the rhomboid is exposed to polarised light, a series of still more interesting phenomena is exhibited. In the position where O vanishes, E is an *orange yellow*, exactly the same as it appeared by common light; and in the position where E vanishes, O is a *yellowish white*, as before. Now it is obvious, that in the first of these positions the image E was not strengthened by the white light of the vanished image O, otherwise the image E would have had the same colour as O + E, or the natural tint of the spar; and that in the second position, the image O had not received the whole of the vanished image E, otherwise it would have had the tint expressed by O + E. It therefore necessarily follows, that a portion of the pencil O has been absorbed in the first position, and a portion of the pencil E in the second. The quantity of light absorbed is a maximum in the two positions where a is 0° and 90°, and is equal to the quantities m and n, which the two images interchange. At different angles with the axis, therefore, it is measured by $\sin.^2 \varphi m$, $\sin.^2 \varphi n$. When this angle is given, the absorbed light varies with the azimuthal angle a, and may be found from the following formula, viz. $T = O \cos.^2 a + E \sin.^2 a$, which supposes that m and n are equal to E and O. Hence when $a = 0°$ $T = O$, or the whole of the pencil E is absorbed. When $a = 45°$, $T = \frac{1}{2} O + \frac{1}{2} E$, or one half of O and E is absorbed, and when $a = 90°$, $T = E$, or the whole of the pencil O is absorbed. When the absorbing crystal is viewed by a doubly refracting prism, the tints of the two pencils Pe and Po will be given by the formulæ $Pe = O \cos.^2 a + E \sin.^2 a$, and $Po = E \cos.^2 a + O \sin.^2 a$.

The property which I have now described as belonging to Calcareous spar, I have found in other twelve crystals with *One axis*. The colour of the ordinary and extraordinary images, or of the absorbed pencils *m* and *n*, is shown in the following table.

List of Absorbing Crystals with One Axis.

Names of Crystals.	Colour when its axis is in the plane of primitive polarisation.	Colour when its axis is perpendicular to that plane.
Zircon	Brownish white	A deeper Brown
Sapphire	Yellowish green	Blue
Ruby	Pale yellow	Bright pink
Emerald	Yellowish green	Bluish green
Emerald	Bluish green	Yellowish green
Beryl blue	Bluish white	Blue
Beryl green	Whitish	Bluish green
Beryl yell. green	Pale yellow	Pale green
Rock crystal, almost transparent	Whitish	Faint brown
Rock crystal yell.	Yellowish white	Yellow
Amethyst	Blue	Pink
Amethyst	Greyish white	Ruby red
Amethyst	Reddish yellow	Ruby red
Tourmaline	Greenish white	Bluish green
Rubellite	Reddish white	Faint red
Idocrase	Yellow	Green
Mellite	Yellow	Bluish white
Phosp. of lime (lilac)	Bluish	Reddish
——————— olive	Bluish green	Yellowish green
Phosphate of lead	Bright green	Orange yellow
Calcareous spar	Orange yellow	Yellowish white

The property which these crystals possess of absorbing the different tints in different positions of the axis, with regard to the plane of primitive polarisation, does not belong to every specimen. There are many crystals of ruby, sapphire, emerald, &c. which give an ordinary and an extraordinary image of the same colour; and whenever this is the case, they are destitute of the property of absorbing polarised light. These two classes of phenomena are indeed invariably connected, and will ultimately be found to have the same origin.

The extreme generality of this property is indicated by the number of crystals in the preceding table, which embraces all the coloured crystals which are at present known to have only one axis of double refraction, excepting *titanite*, *molybdate of lead*, *carbonate of iron and lime*, *arseniate of copper*, certain specimens of *sulphate of nickel*, and *super-acetate* of *copper and lime*, in which I have not detected the property of absorbing polarised light.*

The various coloured minerals which have the cube, the regular octohedron, and the rhomboidal dodecahedron for their primitive form,† are, as might have been expected, destitute of the property of absorption; and I have not been able to discover it in differently coloured glasses, that have received the polarising structure from rapid cooling, or from mechanical compression, or dilatation.

Some of the preceding crystals, such as the *Sapphire* and the *Idocrase* exhibit different colours when common light is transmitted in directions parallel and perpendicular to their axis of double refraction. A specimen of *sapphire* had a *deep blue colour* in one direction, and a *yellowish green* in the opposite direction; and several specimens of *idocrase* had an

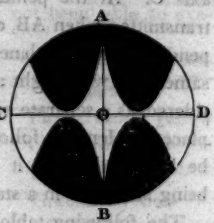
* See the *Phil. Trans.* for 1818, p. 211. † *Id.* p. 254.

orange yellow tint along their axis, and a *yellowish green* tint in a direction perpendicular to their axis. The exhibition of two colours in the same mineral by common light, was first seen in *Iolite*, and the Abbé Haüy had the boldness to give it the name of *Dichroite* from this property, on the presumption that nature had limited it to this mineral. This *dichroism*, as it may be called, which, so far as I know, has never been observed in any other minerals than iolite and mica, is a very common property of crystallized bodies, as will be seen from the following section.

SECT. II. *On the absorption of polarised light by crystals with two axes of double refraction.*

The general phenomena of absorption in crystals with two axes, are nearly the same as those which have been described in the preceding section; but the quantity of light which the ordinary and extraordinary forces interchange, is regulated by new laws depending on the situation of the incident ray with respect to the two axes of double refraction.

If Oo and AB, a line perpendicular to it, are the two axes, and P,P', the resultant axes, or the poles of no-polarisation of *Blue Topaz*: then if COD, the plane of the resultant axes is perpendicular to the plane of primitive polarisation, the polarised light incident on the plate at O will be *blue* after transmission. The blue tint preserves its intensity from O to A and B, the thickness of the plate being supposed to continue the same; but as the incident ray



passes from O to C and D, its intensity diminishes gradually, the light becoming more and more white with a slight tinge of red, till it reaches C and D, where it has the same colour as that of the topaz in common light. As the plate of topaz is turned round the polarised ray, the blue tint changes into white, according to the law given in the preceding section, excepting in the line CD, where the tint is invariably white in every azimuth. When the polarised light is transmitted along any of the resultant axes P, P′, the two tints diverge from the poles in the form of a cross, as shown in the preceding figure.

The effect at O, or the interchange of the blue and white light between the ordinary and extraordinary rays, is related to the axis AB alone; and in like manner the effect at A is related solely to the other axis O. But though the axis Oo is more powerful in topaz than AB, yet the values of m and n for each axis do not appear to be different; a result which might have been expected from the fact, that these tints in different crystals have no relation to the intensity of their polarising forces. The diminution of the tints m and n, in passing from O to C and D, is owing to the action of the other axis O. At the points C and D blue light alone will be transmitted when AB, considered as a separate axis, is perpendicular to the plane of primitive polarisation; and at the same point white light alone will be transmitted when O considered as a separate axis of the same character is similarly placed. Hence it follows, that the transmitted light should be bluish white, as it actually is, the interchanged portions being as it were in a state of equilibrium.

The following table contains the tints m and n in several crystals, in which the effect will be seen in every azimuth.

List of absorbing Crystals with Two axes.

	Plane of the resultant axes in the plane of prim. polarisation.	Plane of the resultant axes perpendicular to the plane of prim. polarisation.
Topaz blue	White	Blue
—— green	White	Green
—— greyish blue	Reddish grey	Blue
—— pink	Pink	White
—— pink yellow	Pink	Yellow
—— yellow	Yellowish white	Orange
Sulphate of barytes		
—— yellowish purple	Lemon yellow	Purple
—— yellow	Lemon yellow	Yellowish white
—— orange yellow	Gamboge yellow	Yellowish white
Kyainte	White	Blue
Dichroite	Blue	Yellowish white
Cymophane	Yellowish white	Yellowish
Epidote olive green	Brown	Sap green
—— whitish gr.	Pink white	Yellowish white
Mica	Reddish brown	Reddish white

The following table shows the characters of m and n in crystals with two axes, which I have not been able to examine in every azimuth.

	Axis of prism in the plane of primitive polarisation.	Axis of prism perpendicular to the plane of primitive polarisation.
Mica	Blood red	Pale greenish yellow
Acetate of copper	Blue	Greenish yellow
Muriate of copper*	Greenish white	Blue
Olivine	Bluish green	Greenish yellow
Sphene	Yellow	Bluish

* The tints are given in relation to the short diagonal of its rhomboidal base.

	Axis of prism in the plane of primitive polarisation.	Axis of prism perpendicular to the plane of primitive polarisation.
Nitrate of copper	Bluish white	Blue
Chromate of lead	Orange	Blood red
Staurotide	Brownish red	Yellowish white
Augite	Blood red	Bright green
Anhydrite	Bright pink	Pale yellow
Axinite	Reddish white	Yellowish white
Diallage	Brownish white	White
Sulphur	Yellow	Deeper yellow
Sulphate of strontites	Blue	Bluish white
——— cobalt	Pink	Brick red
Olivine	Brown	Brownish white

In the last eight crystals of the preceding table the tints are not given in relation to any fixed line.

The following table contains the characters of *m* and *n* in crystals, the number of whose axes I have not yet determined.

Phosphate of iron	Fine blue*	Bluish white
Actinolite	Green	Greenish white
Precious opal	Yellow	Lighter yellow
Serpentine	Dark green	Lighter green
Asbestos	Greenish	Yellowish
Blue carb. of copper	Violet blue	Greenish blue
Octohedrite	Whitish brown	Yellowish brown

Several of the preceding crystals which have a laminated structure, such as mica, epidote, &c. or such as have an imperfect transparency from a defective aggregation of their elementary crystals, frequently exhibit their absorptive qualities, and

* When the axis of the prism was in the plane of primitive polarisation.

also their system of coloured rings, by exposure to common light. The light is, in these cases, analyzed in passing obliquely through the laminæ, in the same manner as if it had been transmitted through a bundle of glass plates.

I shall now conclude this section with a particular account of some very interesting phenomena exhibited by several crystals in the preceding tables.

1. *Super-acetate of copper.* When a prism of this metallic salt is exposed to the solar rays, so that the plane of refraction is perpendicular to the axis of the rhomboidal prism, and the ray passes through the angle of the rhomboid, which is 70°, two distinct images of the sun will be observed; and the one which has suffered the greatest refraction will be *greenish yellow*, while the other will be of a *deep blue* colour. This separation of the two tints is more distinct in some prisms than in others, owing to the manner in which they are cut from the rhomboidal crystal, and in certain points of incidence the two images have the same tint. When a plate of super-acetate of copper is ground so thin as to be transparent, it has a brilliant green colour, composed of blue and greenish yellow. If it is exposed to polarised light, so that the axis of the rhomboidal prism is in the plane of primitive polarisation, the *greenish yellow* light will be entirely absorbed, and the transmitted ray will be of a *deep blue* colour. By turning the plate round the polarised ray, the greenish yellow rays will re-appear, and will gradually regain their former intensity; while the blue rays will in the same proportion be absorbed, till after one-fourth of a revolution the transmitted light will be wholly *greenish yellow*. When the faces of the plate are perpendicular to any of the two resul-

tant axes of the crystal, the blue and the greenish yellow light have the form of a cross, the branches of which diverge from the two poles of no-polarisation.

2. *Augite.* When a plate of yellowish brown augite was exposed vertically to common light, the transmitted pencil had a moderate intensity; when it was inclined to one side, in the plane of one of its neutral axes, the light became more and more intense as the obliquity increased, notwithstanding the increase of thickness in the direction of the ray. By examining the light with a prism of calcareous spar, it was found to be all polarised in a plane perpendicular to the plane of inclination. When the plate was now inclined, from this last position in the opposite direction, the intensity of the light gradually diminished till the plate became absolutely impervious to the strong rays of the sun. The pencil which had formerly vanished when the light was analyzed by Iceland spar, now re-appeared, and gradually increased, becoming more and more *green*, while the other pencil, which became fainter, grew more and more *red*, till at a very great obliquity the one pencil became *perfectly green*, and the other a *deep blood red*. By exposure to strong polarised light, the *red* and the *green* were alternately absorbed, according to the position of the neutral axis with respect to the plane of primitive polarisation.

3. *Dichroite* or *Iolite*. This curious mineral has been long known to exhibit by common light a *deep blue* colour along the axis of the prism, and a *faint yellow* or grey colour in a direction perpendicular to it. I have ascertained that these two colours are those of its ordinary and extraordinary images; and even when seen by common light, that they are

related to its axes of double refraction. Dichroite has two axes of extraordinary refraction, the two resultant axes being inclined 62° 50′ to each other, or 31° 25′ to the axis of the prism. If we cut a plate of dichroite with four parallel faces, each of which is perpendicular to the resultant axes, and is inclined 31° 25′ to the axis of the prism, and expose it either to common or to polarised light, so as to have the plane of its resultant axes perpendicular to the plane of primitive polarisation, we shall observe the branches of blue and white light diverging in a beautiful manner from its poles P, P′. (See the figure in p. 17, which is a very imperfect representation of the phenomenon.) The white light becomes more blue from P and P′ to O, and more yellow from P and P′ to C and D. When the plane of the resultant axes is in the plane of primitive polarisation, the poles P, P′ are marked by spots of white light, but every where else the tint is a deep blue. In the plane CADB, the mineral when seen by common light is yellow mixed with a small quantity of blue, polarised in an opposite plane. From A and B towards P and P′, the yellow image becomes fainter till it changes into blue, and the weak blue image is reinforced by other blue rays till the intensity of the two blue images is nearly equal. The faint blue image increases in intensity as the incident ray approaches from C and D to P and P′. From P and P′ to O one of the images is whitish, and the other deep blue, but the whiteness gradually diminishes towards O, where they are both equally blue.*

* Two crystals of Dichroite which I directed to be cut so as to exhibit these phenomena, are in the cabinet of Thomas Allan, Esq. to whose friendship I have been indebted for several of the minerals noticed in this paper.

4. *Epidote.* The cross formed at the resultant axes of epidote has its diverging branches *brown* and *sap green*, and they are distinctly seen in common as well as in polarised light. The green shades into a pale greenish white as the ray recedes from P and P′ to C and D, where it is no longer divisible into two differently coloured pencils. At O the two pencils are brown and green, and at A and B they are brown and a paler green. The dichroism of the epidote is distinctly marked in common light. Along the axis of the prism, and through two of its parallel faces, its colour is a deep orange, while through the other two parallel faces it is a yellowish green.

5. *Mica.* A specimen of this mineral exhibits the branches diverging from P and P′ exactly in the same manner, whether it is seen by common light, or exposed to a polarised ray with the plane of the resultant axes perpendicular to the plane of primitive polarisation. In these positions the branches PA, PB, &c. are a dark brown, and PC, PO, &c. a brownish white. When the plane of the resultant axes is in the plane of primitive polarisation, the colours of the branches are interchanged. The dichroism of mica is finely exhibited in some of its small and perfect crystals. In some of these its colour by common light is greenish yellow along the axis of the prism, and of a deep garnet colour when the light is transmitted through its faces.

6. *Anhydrite.* This mineral exhibits its dichroism by common light. When the light is transmitted in a direction parallel to the laminæ, its colour is pink; but when it is incident in a direction perpendicular to the laminæ, its colour is a palish yellow slightly tinged with pink.

SECT. III. *On the influence of heat in modifying the absorbent power of crystals.*

Having selected several crystals of *Brazilian topaz*, which displayed no change of colour by exposure to polarised light, I found, that after bringing them to a red heat, or even boiling them in olive oil or in mercury, they experienced such a change in their structure, as to exhibit distinctly the power of absorbing polarised light. I next took a topaz, which had one of its two pencils yellow, and the other pink. By exposing it to a red heat, the heat acted more powerfully upon the extraordinary than upon the ordinary pencil, discharging the yellow colour entirely from the one, and producing but a slight change upon the pink colour of the other.

In the formation of pink topazes by heat, it has always been supposed, that the yellow colour is actually changed into pink; but this is quite a mistake, for the pink colour must previously exist in a state of combination with the yellow, and must either form the colour or exist in the colour of one of the pencils produced by double refraction. The heat does no more than discharge one colour, and leave the other almost unimpaired. This result is of considerable practical importance to the jeweller, as it enables him to determine before hand, whether or not any topaz will receive a pink colour from heat; for if this colour exists in one of its images, which will in general be seen by exposing it to a polarised ray, he may predict with certainty the success of his experiment.

When a topaz possessing a pink tint has been taken

from the fire, it is at first perfectly colourless, and acquires the pink colour gradually in the act of cooling. By exposing it repeatedly to the action of a very intense heat, I have never been able either to remove or to modify this permanent tint.

With the view of ascertaining if the absorbing structure could be induced by heat, I exposed to a white heat several crystals of yellowish calcareous spar. After the action of the fire had been continued for some time, a sort of opalescence, or milky opacity, was induced; and the light, which went to the formation of the ordinary image, was much redder than that which formed the extraordinary ray. This effect I naturally ascribed to some change in the state of the carbonic acid; and upon continuing the action of the heat, and watching the process of decomposition, I found that when the carbonic acid was expelled from a film about the 200th part of an inch thick, its surface was covered with *vesicles arranged in straight lines parallel to the short diagonal of the rhomboid.* These vesicles had, in general, an elliptical form, with a cut or opening in the direction of their transverse axis through which the gas had escaped. After the spar was taken from the fire, a great number of the vesicles burst with a noise similar to that which accompanies the bursting of the indusium of the fern, and carried off a portion of the thin calcareous pellicle. When this pellicle was removed, the subjacent surface was covered with a series of minute parallel grooves inclined about 20° 57′ to the short diagonal. In repeating this experiment, and seizing the proper time for withdrawing the spar from the fire, I

have never failed to observe the fact of the arrangement and bursting of the vesicles, and I have no hesitation in concluding, that the carbonic acid is arranged in planes passing through the axis of the crystal, a result which I had formerly assumed in explaining the phenomena of double refraction. This method of studying the structure of bodies by watching them in the process of disintegration, may be found to have a very extensive application in chemical and mineralogical enquiries.

The observations contained in the preceding pages, indicate in a manner by no means equivocal, that the colouring particles of crystals, instead of being indiscriminately dispersed throughout their mass, have an arrangement related to the ordinary and extraordinary forces which they exert upon light. In some specimens the extraordinary medium is tinged with the same colouring particles, and with the same number of them as the ordinary medium: but in other specimens of the same mineral, the extraordinary medium is either tinged with a different number of particles of the same colour, or with a colouring matter entirely different from that of the ordinary medium. In certain specimens of topaz, the colouring matter of the one medium is more easily discharged than that of the other; and in two specimens of *emerald*, the colouring matter which tinges the ordinary medium in the one, tinges the extraordinary medium in the other, and *vice versa*.

All those crystals in which the colouring matter of the one medium differs either in character or intensity, possess the property of absorbing the two tints according to the laws

already mentioned, but there is also reason to believe, that polarised light suffers the same kind of absorption in those crystals whose two images have the same tint, and even in those which are perfectly colourless.

I have the honour to be, &c.

DAVID BREWSTER.

Edinburgh, Oct. 17th, 1818.

To the Right Hon. Sir Joseph Banks, Bart. G. C. B. &c. &c. &c.

III. *Observations sur la décomposition de l'amidon à la température atmosphérique par l'action de l'air et de l'eau.* *Par* Théodore de Saussure, *Professeur de Minéralogie dans l'Académie de Genève, Correspondant de l'Institut Royal de France, &c. Communicated by* Alexander Marcet, *M. D. F. R. S.*

Read December 17th, 1818.

§ I. L'EXAMEN des changements que les substances végétales subissent en agissant les unes sur les autres, et par leur exposition à l'action de l'air et de l'eau, est le plus sûr moyen d'expliquer plusieurs effets de la végétation; s'il ne conduit pas à ce but, il donne lieu à des expériences importantes pour la théorie de la fermentation.

L'amidon n'a presque pas été examiné sous ce rapport, ou du moins il ne l'a été que par des observations insuffisantes et indirectes; je rappellerai celles qui me sont connues.

Lorsqu'on eut trouvé que les graines céréales formoient du sucre en germant, et que cette production n'avoit pas lieu en même temps à la température atmosphérique, dans des graines privées du contact du gaz oxigéne, et imprégnées d'eau,* on fut conduit à admettre que ce gaz qui disparoit dans la germination pour former de l'acide carbonique avec le carbone de la semence, étoit le principal agent† de la conversion de la matière farineuse en sucre, sans avoir cependant aucune preuve directe de cette théorie; d'ailleurs,

* Some experim. and observ. on the nature of sugar by W. CRUIKSHANKS.

† Ibid, and system of chemistry, by TH. THOMSON.

l'observation que les graines céréales ne forment point de sucre avec l'eau sans le contact de l'air, n'avoit été fondée que sur leur saveur, ou sur un apperçu trop vague pour qu'il put être admis sans un examen ultérieur.

M. VOGEL a recherché l'influence de la chaleur sur l'amidon mêlé d'eau, en le soumettant à l'ébullition avec ce liquide pendant quatre jours de suite. Le mélange est devenu très fluide, il a transmis par la filtration un liquide qui après avoir été évaporé a présenté un mucilage épais et amer qui n'avoit pas le moindre gout sucré. L'amidon resté sur le filtre résistoit à l'action de l'eau bouillante, et présentoit une masse cornée très dure.*

On n'ignore pas que M. KIRCHOFF a découvert dans ces derniers temps qu'en ajoutant du gluten sec pulvérisé à une double quantité d'amidon reduit à l'état d'empois, et qu'en les faisant digérer pendant dix ou douze heures à une température de 40° à 60° R. celui-ci se convertit en partie en sucre.† Ce résultat très intéressant, mais dont les circonstances n'ont pas été suffisamment déterminées, a conduit ce chimiste à admettre que la conversion de l'amidon en sucre dans la germination, s'opère uniquement par le gluten, et à exclure l'explication de ceux qui, avant ses observations, avoient attribué ce changement à l'influence du gaz oxigéne sur la matière farineuse. M. KIRCHOFF appuye sa réfutation en avançant que l'amidon seul, placé dans des circonstances favorables à la germination, ne forme point de sucre.

Les expériences que je vais décrire prouvent cependant que l'empois d'amidon seul et abandonné à lui-même forme au

* Annales de Chimie, T. 82.

† Beitrage zur chemie und physiq. vom SCHWEIGER, 14 Band.

bout d'un certain temps une quantité considérable de sucre crystallisable et qui a beaucoup de rapport avec celui que ce chimiste a obtenu par l'acide sulfurique. Cette décomposition spontanée de l'amidon mêlé d'eau fournit encore d'autres produits, et en particulier une espèce de gomme et une matière intermédiaire entre celle-ci et l'amidon. Leur proportion varie suivant plusieurs circonstances qu'il est très difficile de déterminer. Pour indiquer l'ordre de mes recherches et le procédé de mes analyses, j'exposerai en détail la première que j'ai faite à ce sujet. C'est celle dans laquelle la décomposition de l'amidon a été la plus avancée, quoique ce ne soit pas l'opération qui m'ait fourni la plus grande quantité de sucre.

§ II. J'ai placé de l'empois formé avec vingt grammes d'amidon,* et douze fois son poids d'eau distillée bouillante dans un vase cylindrique où le mélange offroit une grande surface à l'air, et formoit une couche de deux centimètres d'épaisseur. Ce vase recouvert d'un grand récipient sous le quel l'air extérieur pénétroit facilement, a été laissé en repos pendant deux ans dans un lieu où la température s'élevoit en

* L'amidon employé à cette expérience étoit de l'amidon de froment, pris dans le commerce sous le nom d'amidon de *première qualité*. On n'en pouvoit pas séparer une quantité notable de gluten : car après avoir mis cet amidon en macération pendant dix jours avec trente fois son poids de vinaigre distillé, il a fourni par la filtration une liqueur qui étant évaporée a laissé un résidu acide, mol, dont le poids n'étoit que la quatre millième partie de l'amidon employé ; et ce résidu retenoit un peu d'eau, le mucilage du vinaigre, et environ la sixième de son poids d'acide acétique. L'eau froide mise en macération pendant vingt quatre heures sur cet amidon, ne lui enlevoit pas une quantité sensible de son poids. 100 parties de cet amidon séché à 18° R. perdoient par le dessèchement à la temperature de l'eau bouillante 13,66 de leur poids. J'ai varié mes expériences avec des amidons pris dans différents magasins ; ils m'ont tous fourni des résultats analogues à ceux que je décris dans le texte.

été jusqu'à 18° R. Lorsqu'à cette époque, le mélange a été retiré, il a présenté une pâte grise, liquide, couverte de moisissure, et presque sans odeur ; elle ne changeoit alors, ni dans aucune époque antérieure de son altération, ni après avoir été délayée et filtrée, les couleurs végétales employées comme réactifs ; elle ne pouvoit plus faire les fonctions de colle. Le poids de l'amidon avant son altération étoit beaucoup plus grand que celui de cet amidon altéré ; le premier étoit au second comme 100 : 76,2, après leur dessèchement à la température atmosphérique, et comme 100 : 180,46, après leur dessèchement à la température de l'eau bouillante.

§ III. La pâte dont je viens de parler a été mise en macération pendant vingt quatre heures avec une quantité d'eau *froide* à peu près égale à vingt fois le poids de l'amidon considéré dans l'état sec, puis jetée sur un filtre de papier, et lavée avec une nouvelle dose de liquide ; elle a fourni une liqueur transparente, d'un jaune pâle, qui a laissé après son dessèchement un extrait un peu mol, et dont le poids étoit égal à la $\frac{47}{100}$eme soit environ à la moitié de celui de l'amidon qui avoit formé l'empois. Cet extrait a été dissous dans une fois son poids d'eau, puis mêlé avec dix fois son poids d'esprit de vin à 35° de l'aréomètre de Baumé : il a dissout le sucre en précipitant l'espèce de gomme que je décris dans la note A à la fin de ce mémoire. Cette gomme retenoit alors un peu de sucre et d'un produit intermédiaire entre la gomme et l'amidon ; on l'a purifiée en la traitant par l'alcool comme l'extrait précédent, en la dissolvant ensuite dans l'eau, et en la filtrant. Le poids de cette gomme dessèchée, transparente, et soluble dans l'eau froide en toute proportion, étoit égal à deux grammes, ou à la dixième de l'amidon mis en expérience.

§ IV. Les dissolutions alcooliques évaporées d'abord à un feu doux et ensuite à la température atmosphérique ont commencé par présenter un résidu sucré, transparent, en consistance de miel, qui retenoit encore un peu de gomme que j'ai comprise dans le produit précédent; elle a été séparée par une nouvelle dissolution dans l'eau, et la précipitation par l'alcool. Il s'est formé au bout de peu de jours dans le résidu de l'évaporation de la liqueur alcoolique, des crystaux le plus souvent réunis en groupes sphériques, hérissés de lames transparentes; ils présentoient au microscope lorsqu'ils étoient isolés, des lames quarrées et des cubes. Bientôt ce résidu s'est presque entièrement converti en une masse opaque de sucre concret, jaunâtre, doué de l'odeur propre à la cassonade: il est resté long-temps gluant par son mélange avec un syrop plus difficile à crystalliser; mais par une longue exposition à l'air, le tout a paru sec et homogène. Son poids étoit égal à la 0,37ème, soit à plus du tiers de l'amidon employé pour cette expérience.

Ce sucre que je n'ai pas dépouillé de son principe colorant passe à la fermentation alcoolique avec une très petite quantité de levure; si elle étoit trop abondante ou si elle montoit à $\frac{1}{5}$ème du sucre, ce mode de décomposition n'auroit point lieu.

100 parties d'alcool absolu bouillant en dissolvent 5 ou 6 parties. L'alcool à 35° Baumé en dissout $\frac{1}{4}$ème de son poids à une température de 20° R. si le sucre est dans l'état sec ou entièrement concréfié, car s'il étoit un peu visqueux par son mélange avec une matière plus difficile à crystalliser, cet alcool en dissoudroit une plus grande quantité.

Ce sucre s'est liquéfié à la température de l'eau bouillante; il a perdu par cette opération entre la 0,07 et la 0,08 de son

poids. Comme ses principales propriétés conviennent au sucre d'amidon préparé par l'acide sulfurique, il est très probable que ces deux substances sont identiques.

§ V. Après avoir extrait par l'eau froide, les produits gommeux et sucrés contenus dans le résidu de la décomposition de l'amidon, je l'ai soumis deux fois pendant une ou deux minutes à l'ébullition avec une quantité d'eau égale à celle qui avoit été employée dans l'opération précédente. Les décoctions filtrées après leur *réfroidissement* ont fourni par l'évaporation à siccité, un résidu fragile, jaune, à demi-transparent; son poids étoit égal à la septième partie de l'amidon employé. Cette substance sur laquelle je reviens plus en détail dans la note B à la fin de ce mémoire, a des propriétés intermédiaires entre le principe gommeux précédent et l'amidon: elle se dissout en petite quantité dans l'eau froid, et en toute proportion dans l'eau bouillante, en formant avec l'une et l'autre, des dissolutions transparentes, non gélatineuses, et qui se filtrent facilement au travers du papier. J'ai donné ici, pour éviter les périphrases, le nom de *amidine* à ce produit, soit aux modifications de l'amidon dans lesquelles il acquiert la propriété de se dissoudre dans l'eau froide, en conservant la faculté de colorer en bleu la solution aqueuse de iode.

§ VI. La pâte qui est restée sur le filtre après l'action de l'eau bouillante, sur l'empois altéré, pouvoit dès lors faire les fonctions de colle; elle étoit presque noire, et dans l'état sec son poids étoit égal à la sixième partie de l'amidon mis en expérience. L'éther ou l'alcool absolu mis en digestion sur ce résidu, y a dissout en partie une matière colorante, brune, qui en étoit précipitée par l'eau. Cette matière colorante desséchée s'est présentée sous l'apparence d'une huile ou d'une

résine épaisse et visqueuse, mais trop peu abondante pour que j'aie pu la mieux examiner. Elle n'équivaloit qu' à trois millièmes de l'amidon employé.

§ VII. On sait qu' une partie d'amidon se dissout en peu de minutes à l'aide d'une douce chaleur, et sans décomposition apparente, dans quarante fois son poids d'acide sulfurique délayé ou composé d'une partie d'acide sur douze d'eau: mais cette liqueur soumise à l'ébullition dans les mêmes proportions avec le résidu amilacé pulvérisé, sur lequel l'eau bouillante et l'alcool n'avoient plus d'action, n'en a pu dissoudre que la 0,35e partie, soit environ le tiers. La partie dissoute qu'on pouvoit précipiter en partie par l'alcool,* étoit de l'amidon non décomposé mêlé d'une petite quantité d'amidine.

§ VIII. La matière insoluble par l'acide sulfurique délayé se montroit après son dessèchement, sous l'apparence de grumeaux opaques, très fragiles; ils se sont dissous facilement (sauf un petit résidu composé de ligneux et de charbon)

* L'amidon forme avec l'acide sulfurique une combinaison qui crystallise en aiguilles transparentes, prismatiques, très fines ou très allongées. Pour obtenir ce produit, on précipite par de l'alcool la dissolution d'amidon dans l'acide sulfurique délayé, on lave avec de l'alcool le précipité, qui est un mélange d'eau, d'acide sulfurique, d'amidon pur et de la susdite combinaison: elle se dissout en partie par une petite quantité d'eau froide. Cette dissolution filtrée fournit par une évaporation lente et spontanée les crystaux dont j'ai parlé, qui sont melés avec de l'acide sulfurique libre, qu'on enleve par leur lavage avec de l'alcool. Ces crystaux sont en partie décomposés par l'eau qui en précipite de l'amidon; mais en filtrant la dissolution aqueuse, en évaporant, et en enlevant avec de l'alcool, l'acide sulfurique mis à nud, on obtient de nouveau la combinaison ou séche et crystallisée d'acide sulfurique et d'amidon. Ce dernier aussi précipité de cette combinaison par l'eau, a subi une légère altération, car il ne peut précipiter qu'en rouge de vin la solution aqueuse d'iode.

à l'aide d'une douce chaleur, dans dix fois leur poids d'une lessive de potasse qui contenoit un douzième de cet alcali; ils ont formé ainsi une solution brune, très liquide, qui n'avoit point la consistance visqueuse et gélatineuse propre aux solutions alcalines d'amidon, et ils en étoient précipités par l'acide sulfurique délayé, sous la forme d'une poudre combustible, jaune, légère, qui après son dessèchement offroit une masse noire, brillante semblable à du jayet. Cette dernière délayée dans l'eau, coloroit encore en bleu la solution aqueuse de iode.

La substance végétale dont cette matière amilacée insoluble à chaud par l'acide sulfurique délayé, se rapproche le plus, est le ligneux ou le bois: elle en diffère cependant en ce qu'elle est soluble dans des lessives de potasse plus étendues que celles qui peuvent dissoudre ce dernier, et en ce qu'elle colore en bleu la solution aqueuse d'iode. Je désignerai cette matière sous le nom de *ligneux amilacé*.

§ IX. Le charbon mêlé de ligneux que la lessive alcaline précédente n'a pu dissoudre équivaloit à $\frac{1}{20}$ème de l'amidon employé. Ils n'ont laissé après leur combustion, qu'une très petite quantité de cendres.

§ X. Pour reconnoître le genre d'altération que l'air avoit éprouvé pendant la formation de tous les produits précédents, j'ai exposé sous des récipients pleins d'air et fermés par du mercure, de l'empois récent d'amidon, et d'autre part, de l'empois à différentes époques de son altération par l'action antérieure de l'air. Dans toutes ces expériences, le volume de l'air renfermé dans les récipients n'a subi aucun changement; le gaz oxigène en a été en partie détruit, mais il a été remplacé par un volume égal de gaz acide carbonique. L'altération que l'amidon a fait subir ainsi à l'air ne s'est

opérée que lentement; dans le cas où elle étoit la plus rapide, et où j'ai employé de l'empois récent, quinze grammes de cette substance contenant $\frac{1}{12}$ème d'amidon n'ont formé sous une grande surface * pendant deux mois à environ 18° R. que cinquante centimètres cubes de gaz acide carbonique dans un décimètre cube d'air. La même quantité d'empois après deux ans d'exposition à l'air libre, produisoit dans une expérience semblable, un volume de gaz acide égal au quart du précédent. Ces expériences montrent que l'influence du gaz oxigène sur l'amidon se borne à lui enlever du carbone. J'ai vu de plus, en même temps, que la perte de poids que l'amidon altéré éprouve après son dessèchement est beaucoup plus grande que celle qui résulte de la soustraction de ce carbone. On peut en conclure que l'amidon en s'altérant à l'air, perd sous forme d'eau une grande proportion de son oxigène et de son hydrogène. Le carbone enlevé à l'amidon par l'air étoit à l'eau qui se formoit en même temps dans le rapport de 1 : 74, pendant les deux premiers mois de l'altération.

§ XI. La colle d'amidon laissée en repos à l'air libre se couvre de moisissure, et il étoit possible que les résultats que j'ai obtenus fussent l'effet de cette végétation; j'ai empêché son développement, soit en agitant l'empois tous les jours, soit en le plaçant dans de grandes jares fermées, pleines d'air, qui avoit été exposées à la température de l'eau bouillante, immédiatement avant l'introduction de l'amidon, et je n'en ai pas moins obtenu tous les produits dont j'ai parlé précédem-

* Lorsque l'empois n'offre pas une grande surface à l'air, il dégage du gaz acide carbonique à la formation duquel cet air n'a aucune part, et alors on n'observe plus d'égalité entre les volumes du gaz oxigène consumé et du gaz acide carbonique porduit.

ment, savoir, du sucre, de la gomme, de l'amidine, une substance d'apparence huileuse, une matière ligneuse, de l'eau, du charbon, et enfin du gaz acide carbonique dont l'oxigène appartient à l'air ambiant.

§ XII. En répétant les expériences précédentes, soit avec de l'amidon de froment, soit avec celui de pomme de terre, dans différentes circonstances, et toujours avec le contact de l'air, j'ai obtenu les mêmes produits ; leur proportions seules ont varié. Elles m'ont paru indiquer, 1° qu'il se formoit moins d'eau lorsque l'empois offroit moins de surface à l'air ; 2° qu'une température un peu plus élevée que celle qui avoit été employée dans ma première expérience favorisoit beaucoup la production du sucre ; 3° que tout celui qui s'étoit produit dans cette épreuve n'avoit pas été recueilli parce qu'il s'étoit détruit par une fermentation trop prolongée.

§ XIII. Pour déterminer d'une manière précise si l'empois d'amidon se décompose sans le contact de l'air en formant du sucre, j'ai rempli, à la réserve de 8 centimètres cubes, une bouteille avec 300 centim. cubes d'empois, § II; Il a été préparé dans ce vase qui a été bouché et mastiqué avec du ciment pendant que l'eau étoit chaude, pour expulser l'air contenu dans le petit espace désigné plus haut. Cette bouteille a été placée en été dans une chambre où la température s'est maintenue entre le 18° et le 20° R.

J'ai exposé en contact avec l'air dans un vase ouvert, très évasé, à côté du précédent, du même empois qui a été agité tous les jours avec une spatule, et auquel on a ajouté successivement de l'eau distillée pour remplacer celle qui s'évaporoit. Il ne s'y est point formé de moisissure, et il est devenu en peu de temps tout à fait liquide.

Au bout de 38 jours, j'ai analysé l'empois altéré, contenu dans les deux vases. Il avoit perdu l'odeur propre à l'amidon récent, et n'en avoit point contracté de bien marquée. Il s'étoit dégagé dans la bouteille fermée un air condensé qui s'est échappé à son ouverture avec une sorte d'explosion, et que j'ai trouvé par des expériences subséquentes être du gaz acide carbonique mêlé de gaz hydrogéne.

Après avoir réduit par l'agitation les deux empois en une consistance uniforme, et les avoir pesés, j'en ai séparé une partie déterminée pour la sécher et juger ainsi du changement de poids que l'amidon avoit subi. Ils m'ont fourni des résultats très différents.

L'amidon avoit diminué de poids par la fermentation *avec* le contact de l'air dans le rapport de 100 83 avec des dessèchements opérés à la température de l'eau bouillante.

L'amidon a été *sans* le contact de l'air n'avoit subi, après son dessèchement au même degré, aucune diminution de poids; il paroissoit même avoir augmenté de $\frac{1}{500}$ème. Cette augmentation se confond avec les erreurs d'observation; mais si l'on considère que pendant la fermentation, il a formé du gaz acide carbonique dont je n'ai pas tenu compte, et qu'il a diminué de poids, en se décomposant et en produisant de l'eau pendant un dessèchement* qui a duré deux ou trois

* Les dessèchements avant et après la fermentation, ne sont pas faits dans des circonstances égales. Le premier s'opère sur une matière déjà sèche en apparence et inaltérable à l'air par l'eau qu'elle récéle. Le second est fait sur une substance réduite en pâte et très altérable à l'air dans l'eau qui l'environne. J'ai préparé de l'empois avec 100 parties d'amidon seché à 80° R.; cette pâte réduite par l'évaporation à l'état sec sous la température précédente, n'a représenté par son poids que 98,5 parties d'amidon. Ce résultat indique qu'une substance analogue, dont le poids dans l'état sec ne se seroit point trouvé changé après un pareil traitement, auroit

jours avec le contact de l'air, l'on doit être porté à admettre que cette fécule s'est assimilé les éléments de l'eau dans la fermentation sans le contact de l'air.

Lorsque les dessèchements étoient faits à la température de 18° R. l'amidon paroissoit avoir diminué de poids par la fermentation sans le contact de l'air, dans le rapport de 100:95,8; mais cette diminution étoit illusoire parce qu'il retenoit plus d'eau hygrométrique avant sa fermentation; 100 parties d'amidon non fermenté perdoient 18,66 d'eau par un dessèchement à 80° R.; et 100 d'amidon fermenté ne perdoient que 9,87 par la même opération.

Cent parties d'amidon de froment seché à 18° R. ont laissé par leur fermentation spontanée avec l'eau, dans 38 jours, sans le contact de l'air, un résidu qui contenoit après son dessèchement à la température précédente,

§ IV. Sucre - -	47,4.
§ IV. et § III. Gomme -	23.
§ III. et § V. Amidine -	8,9.
§ VIII. Ligneux amilacé -	10,8.
§ IX. Ligneux mêlé de charbon - - -	quantité imponderable.
§ VII. Amidon non décomposé	4.
	93,6.

Cent parties d'amidon de froment ont fourni par leur fer-

accru sa matière solide avant son dessèchement; et c'est ce qui a eu lieu avec l'amidon fermenté sans le contact de l'air.

Tous ces dessèchements ont été commencés à une chaleur de 40° R. et achevés à la température atmosphérique. La substance a dèslors été pulverisée, puis placée dans la cucurbite d'un alembic recouvert de son chapiteau, et dont le bain marie étoit tenu en ébullition pendant sept heures.

mentation avec le contact de l'air et dans des circonstances d'ailleurs égales,

Sucre - - -	49,7.
Gomme - - -	9,7.
Amidine - - -	5,2.
Ligneux amilacé - -	9,2.
Ligneux mêlé de charbon -	0,8.
Amidon non décomposé, -	3,8.
	77,9.

Les produits avec le contact de l'air étoient tous plus foncés en couleur.

J'ai répété les deux dernières expériences avec de l'amidon de pomme de terre que j'ai extrait avec beaucoup de soin. Il n'abandonnoit rien de soluble à l'eau froide. Cet amidon qui suivant M. KIRCHOFF, est toujours dépourvu de principe glutineux, diffère à quelques égards de l'amidon de froment; le premier est plus friable; il est composé de grains ovoides environ deux fois plus gros; il exige une température un peu moins élevée pour se réduire en gelée avec l'eau; il peut se dissoudre dans des lessives de potasse plus délayées; il se décompose moins promptement par la fermentation spontanée; il contient plus d'eau hygrométrique. 100 parties de cet amidon seché à 18° R. et 88° de l'hygrom. à cheveu ont perdu par le dessèchement à la température de l'eau bouillante 16,41 d'eau, tandis que l'amidon de froment a perdu par le même procédé 13,66.

Les résultats suivants ont été obtenus à une époque différente de la précédente, mais dans des circonstances presque sem-

blables, c'est-à-dire à une températ. de 18° à 20° R. : J'ai mastiqué seulement à la bouteille où se faisoit la fermentation sans le contact de l'air, un tube recourbé pour recueillir sur le mercure le gaz qui se dégageoit. 30 grammes de cet amidon reduit à l'état d'empois avec 360 grammes d'eau, ont dégagé, dans 42 jours, 26 centimét. cubes de fluide aëriforme qui étoit composé en volume de 80 parties de gaz hydrogéne presque pur (voyez la note D, à la fin de ce mémoire), et de 16 parties de gaz acide carbonique. Quoique ce dernier paroisse le moins abondant, il n'est pas douteux qu'il n'ait été produit en quantité environ quatre fois plus grande que le gaz hydrogène, parce que le volume du liquide qui étoit presque quatre fois plus grand que le gaz dégagé, a retenu le gaz acide carbonique dont je n'ai pas tenu compte, et a émis au contraire presque tout le gaz hydrogéne.

Par la fermentation *en contact* avec l'air, l'amidon de pomme de terre a diminué de poids en raison de 100 : 77,7 avec des dessèchements à la température atmosphérique, et en raison de 100 : 85,3 par des dessèchements à la température de l'eau bouillante.

Le poids de l'amidon altéré *sans* le contact de l'air, avec des dessèchements à la température de l'eau bouillante, étoit précisement égal au poids du même amidon avant sa fermentation, en ne tenant pas compte de la perte de poids qu'il a subie par le dégagement du gaz acide carbonique, ni de celle qu'il a éprouvée après cette fermentation par sa décomposition au contact de l'air pendant le dessèchement. L'amidon a paru avoir diminué de poids dans le rapport de 100 : 94 avec des dessèchements à 18° R. ; mais ce changement n'étoit

dû qu'à la différente faculté hygrométrique de cette substance avant et après son altération. Dans ce dernier état, 100 d'amidon perdoient 10,6 d'eau à 80° R. tandis que cette perte montoit à 16,41 avant la fermentation.

100 d'amidon de pomme de terre séché à 18° R. ont laissé par la fermentation spontanée pendant 40 heures, sans le contact de l'air, un résidu qui a fourni après son dessèchement à la même température,

Sucre - - -	85,4.
Gomme - - -	17,5.
Amidine - - -	18,7
Ligneux amilacé - -	7
Ligneux mêlé de charbon quantité imponderable. - - -	
Amidon non décomposé -	9,4.
	88.
Perte dans l'analyse	6.
	94.

100 d'amidon de pomme de terre ont fourni par leur fermentation avec le contact de l'air, et dans des circonstances d'ailleurs égales,

Sucre - - -	80,4.
Gomme - - -	17,2.
Amidine - - -	17.
Ligneux amilacé - -	4,4.
Ligneux mêlé de charbon -	0,2.
Amidon non décomposé -	9,3.
	78,5.

Les principaux résultats que je déduis de ces expériences

sont ; 1°, que l'air n'a aucune influence sur la formation du sucre dans la décomposition spontanée de l'amidon ; 2°, que la fermentation sans le contact de l'air diffère de celle qui s'opère avec ce contact, en ce que dans cette dernière, l'amidon perd sous forme d'eau une grande proportion de son oxigène et de son hydrogène ; tandis que dans la fermentation sans le contact de l'air, l'amidon bien loin de perdre de l'eau paroit s'approprier au contraire une petite quantité des éléments de ce liquide.*

§ XIV. Pour comparer la fermentation spontanée de l'amidon avec le procédé par lequel M. Kirchoff a produit du sucre dans l'espace de dix ou douze heures en mêlant du gluten sec pulvérisé avec une double quantité d'amidon qu'on réduit à l'état d'empois, et en les exposant à une température de 40° à 60° R., j'ai divisé ce mélange en trois parties égales qui ont été chauffées (avec le degré et le temps prescrits) au même bain marie, dans trois vases différents : Le premier d'entr'eux étoit plein et exactement fermé ; le second étoit ouvert, évasé, et en libre contact avec l'air ; le troisième étoit un ballon fermé plein d'air, dont l'empois n'occupoit que la cinquantième partie.

Le poids du produit de ces opérations, même de celles qui avoient été faites avec le contact de l'air, étoit égal aux poids du gluten et de l'amidon avant leur mélange, ou du moins il ne leur étoit inférieur que d'une quatre millième, en faisant les dessèchements à la température de l'eau bouillante.

Dix grammes d'amidon ont produit dans le ballon fermé

* Le sucre obtenu dans les expériences où la fermentation de l'amidon de froment et de pomme de terre n'a duré que cinq ou six semaines, n'a point pu crystalliser. Il n'en étoit pas de même lorsque les fermentations ont été beaucoup plus prolongées.

et plein d'air, 50 centimètres cubes de gaz acide carbonique à la formation duquel cet air n'avoit eu aucune part, car après son mélange avec la potasse, il contenoit la même proportion de gaz oxigéne qu'avant l'expérience.

Il s'est formé dans les trois vases à très peu près la même quantité de sucre, c'est-à-dire environ la septième partie de l'amidon employé. Les petites différences entre les résultats, et qui pouvoient être accidentelles, se trouvoient en faveur de l'exclusion de l'air pour la production du sucre. La matière sucrée bien purifiée et obtenue par les procédés du gluten diffère du sucre qu'on obtient de l'amidon fermenté sans mélange étranger,

1°, En étant beaucoup moins soluble dans l'alcool aqueux. 100 parties de cette liqueur à 35° de l'aréomètre de Baumé n'en ont pu dissoudre que 2,85 parties à une température de 18° R.

2°, En ce qu'elle forme avec dix fois son poids d'eau une dissolution où la décoction de noix de gale produit un précipite blanc, très abondant, et dont on ne voit aucun vestige avec les dissolutions des autres sucres.

La gomme obtenue par le procédé décrit § 3, équivaloit à la quinzième partie de l'amidon employé ; elle différoit de celle de l'amidon fermenté, 1°. en formant une dissolution aqueuse qui, en opposition avec le résultat de M. Kirchoff, étoit abondamment troublée par la décoction de noix de gale ; 2°, en colorant en bleu la solution aqueuse de iode qui y manifestoit ainsi la présence de l'amidine ou de l'amidon.

Après avoir extrait par l'eau froide les principes gommeux et sucrés, j'ai essayé inutilement de séparer par l'eau bouillante, l'amidine du résidu de l'opération ; il se réduisoit par

ce traitement en une colle qui ne transmettoit rien par les filtres. La matière inattaquable par l'eau froide se dissolvoit en partie dans l'acide sulfurique délayé. Le résidu indissous étoit un mélange de gluten et de ligneux amilacé. Ce dernier se séparoit dans la liqueur par sa différente densité; il ne donnoit par sa blancheur aucun indice de matière charbonneuse, et il se dissolvoit en entier dans des lessives de potasse très étendues. Quoique l'on reconnut dans ces produits tous ceux de la fermentation de l'amidon seul, ils étoient impossibles à extraire dans leur état de pureté à cause de l'intervention de la matière glutineuse qui les faisoit adhérer les uns aux autres.

Il se forme dans cette opération, suivant l'observation de M. Kirchoff, un acide que la liqueur retient après l'ébullition. Ce produit est dû au gluten qui le dégage par sa fermentation, sans le secours de l'amidon. L'absence de cet acide dans la fermentation de l'amidon pur, montre que la formation du sucre par les procédés antérieurs n'a pas été due à la présence accidentelle du gluten.

Malgré ces différences, la décomposition spontanée de l'amidon seul sans le contact de l'air, et celle qui s'opère par l'intermède du gluten, ont dans leurs résultats généraux plusieurs caractères semblables très frappants. Il y a production de matière sucrée, de gomme, de ligneux amilacé, et probablement d'amidine. Il y a dégagement de gaz acide carbonique. Il n'y a point de précipitation de charbon et point d'eau formée ou de perte de poids dans le produit sec de l'opération. On peut en conclure que le gluten en s'unissant à l'amidon ne fait qu'accélérer une fermentation que ce dernier auroit subie par lui-même sans cette influence, qui modifie légèrement les produits de l'opération.

§ XV. M. Kirchoff a trouvé* que quelques acides différents de l'acide sulfurique convertissoient l'amidon en sucre. Pour connoître si le gaz acide carbonique qui se forme par la fermentation de l'amidon avoit été la cause de la production du sucre dans mes expériences, j'ai introduit dans un grand ballon de verre, de l'empois d'amidon de froment, § 11, dont il n'occupoit que la cinquantième partie, et après y avoir fait le vide par la pompe pneumatique, je l'ai rempli avec du gaz acide carbonique pur. Ce ballon, fermé par un robinet, a été exposé pendant quarante jours dans un lieu où la température s'est maintenue entre le 18° et le 20° R. L'amidon n'a presque pas été décomposé, car 100 parties ont fourni par cette opération,

Sucre - - -	1.
Gomme - - -	0,86.
Amidine mélé d'amidon -	1.

Les $\frac{1}{100}$ème de l'amidon ne paroissoient avoir subi aucune altération. Le gaz acide carbonique met donc obstacle à la fermentation de l'amidon, et à la formation de tous les produits dont j'ai parlé.

Résumé des principales observations contenues dans ce Mémoire.

L'amidon réduit par l'eau à l'état d'empois, et abandonné à sa décomposition spontanée, à une température entre 16° et 20° R., produit soit avec le contact de l'air, soit sans cette influence,

1°, une espèce de sucre semblable à celle qu'on obtient de

* Journal de Physique par De La Metherie, T. 74.

la même fécule par l'intervention de l'acide sulfurique délaye et d'une plus haute température.

2°, une espèce de gomme qui a un grand rapport avec le principe gommeux de l'amidon torréfié.

3°, une matière que j'ai désignée sous le nom d'amidine, et dont les propriétés sont intermédiaires entre celles de l'amidon et de la gomme précédente.

4°, une substance qui s'approche du ligneux par son insolubilité dans l'eau bouillante et dans plusieurs acides ; mais elle tient de la nature amilacée en colorant en pourpre la solution aqueuse d'iode.

La décomposition spontanée de l'amidon fournit encore d'autres produits ; mais leur présence et le mode de leur formation sont subordonnés à l'action ou à l'absence de l'air atmosphérique pendant la fermentation.

Lorsque cette décomposition se fait *avec* le contact de l'air, l'amidon produit une grande quantité d'eau, dans laquelle le gaz oxigène atmosphérique n'entre point comme principe constituant. Il se forme du gaz acide carbonique dont l'oxigène appartient à l'air atmosphérique. L'amidon dépose encore dans cette circonstance du charbon qu'on ne sépare qu'imparfaitement, et qui rembrunit tous les produits de l'opération. Le gaz oxigène n'est point absorbé dans cette fermentation qu'en tant qu'il forme le gaz acide carbonique dont je viens de parler. Le poids du résidu sec de la décomposition de l'amidon avec le contact de l'air pèse moins que l'amidon employé. La soustraction dû carbone par l'air n'entre que très peu dans ce déchet qui est du presque uniquement à l'eau formée par l'amidon, et qui se volatilise.

Lorsque la décomposition spontanée s'opère *sans* le contact

de l'air, l'amidon ne produit point d'eau, il dégage une petite quantité de gaz acide carbonique et du gaz hydrogène pur ou presque pur. Il ne dépose point de charbon. Le poids du résidu de cette fermentation après le dessèchement à la température de l'eau bouillante s'est trouvé dans mes expériences égal au poids de l'amidon employé à la même température : mais comme je n'ai tenu compte ni de la perte qu'il a subie par le dégagement du gaz acide carbonique, ni de celle qu'il a éprouvée par sa décomposition dans un long dessèchement avec le contact de l'air, il me paroit probable que l'amidon dans sa fermentation sans ce contact, fixe ou s'approprie en petite quantité les éléments de l'eau.

Mes expériences sans l'influence de l'air n'ont été ni assez prolongées ni assez multipliées, pour indiquer si sa présence augmente la quantité du sucre ; leurs résultats à cet égard ont varié. Il est probable que l'air la diminue, en détruisant tous les produits de l'opération.

La conversion de l'amidon en sucre par l'intervention du gluten, dans l'espace de quelques heures, et par une température élevée, fournit des produits sucrés et gommeux qui diffèrent des substances obtenues dans l'opération précédente, en ce qu'ils donnent avec l'eau, des dissolutions où la décoction de noix de gale indique la présence de la matière glutineuse par des précipités abondants. Ce principe donne au produit sucré d'autres propriétés distinctives très saillantes. Il s'engendre de plus dans l'empois mêlé de gluten, un acide qui ne se manifeste point dans la fermentation de l'amidon seul, et qui paroit dû exclusivement à la fermentation du gluten. D'ailleurs la décomposition spontanée de l'amidon

sans le contact de l'air, et celle qui s'opère par l'intermède de la matière glutineuse, ont en général des caractères semblables. Le gluten en s'unissant à l'amidon, ne paroit qu'accélérer une décomposition que celui-ci auroit subie plus tard, sans cette influence.

Fourcroy à désigné quelques opérations chimiques dans les quelles il se produit du sucre, sous le nom de fermentation saccharine. Il avoit principalement fondé cette distinction sur le gout sucré que prennent plusieurs fruits par la coction, et sur la formation du sucre dans l'acte même de la végétation et de l'animalisation: mais le premier résultat, celui de la saveur, étoit trop indéterminé; et le second ne s'adaptoit pas au nom de fermentation, qui suppose l'acte d'un mouvement spontané et intestin dans des substances végétales ou animales désorganisées et privées de vie. Aussi cette désignation n'a-t-elle pas été adoptée. Mais puisque nous voyons par des effets précis que la formation du sucre a lieu dans le sens le plus strict attaché au mot de fermentation, il convient de distinguer cette dernière, et de la faire précéder toutes les autres, en lui conservant le nom de fermentation saccharine.

Genève ce 7 Octobre, 1818.

Note A, *sur la gomme produite par la fermentation spontanée de l'amidon*, § III. *et* § XIII.

Cette gomme purifiée par sa dissolution dans l'eau, sa filtration, et sa précipitation par l'alcool, est, après son desséchement, transparente et presque sans couleur, si la fermentation

s'est faite sans le contact de l'air ; mais si cette opération a été très prolongée avec ce contact, si elle a été accompagnée de moisissure, la gomme est jaune et un peu trop molle pour pouvoir être pulvérisée. 100 parties de cette gomme à 15° R. perdoient par ce dessèchement à la température de l'eau bouillante 11,75 d'eau ; dans cet état, elle est toujours très friable. Elle n'attire pas d'ailleurs l'humidité de l'air ; elle y est inaltérable : mais sa dissolution aqueuse s'y décompose au bout d'un certain temps, sans passer à l'état acide, en prenant une odeur putride, et en déposant d'épaisses mucosités.

Elle est insoluble dans l'alcool, et elle est soluble dans l'eau en toute proportion. Deux parties de ce liquide et une de gomme offrent une solution très fluide, mais elle devient filante et visqueuse lorsque le poids de la gomme excède celui de l'eau.

La dissolution d'une partie de gomme dans dix d'eau n'est troublée ni par l'acetate de plomb, ni par le sous acétate de plomb, ni par la décoction de noix de gale, ni par la liqueur des cailloux.

Elle n'altère pas la couleur de l'infusion de tourne-sol.

Elle ne produit aucun changement de couleur dans la solution aqueuse d'iode.

Elle est foiblement troublée par l'eau de baryte.

Elle ne produit point d'acide muqueux avec l'acide nitrique.

Cette gomme a beaucoup de rapports avec celle qu'on obtient de l'amidon torréfié. Elles diffèrent seulement en ce que l'eau de baryte fait un précipité beaucoup moins abondant dans la solution de gomme d'amidon fermenté ; en ce que cette dernière a un peu de flexibilité à une basse température,

et en ce qu'elle a une couleur beaucoup moins foncée, et presque nulle, si la fermentation s'est faite sans le contact de l'air.

Note B, *sur la matière intermédiaire entre la gomme et l'amidon, ou sur l'amidine*, § V, *et* § 13.

Le résidu de la décomposition spontanée de l'amidon, après avoir été traité par l'eau froide, abandonne à l'eau bouillante un principe que ce liquide, après son réfroidissement et sa filtration, retient en dissolution, et que j'ai désigné sous le nom d'amidine. On la purifie dans l'état sec, en la lavant avec une petite quantité d'eau froide, en la faisant dissoudre dans l'eau bouillante, et en filtrant la dissolution après son réfroidissement. Ce produit ne me paroissant être qu'une modification spéciale de l'amidon, ne conservera pas le nom que je lui ai donné ici pour abréger : car on ne surchargera pas la science de nouveaux noms pour les modifications infinies que peut présenter toute substance végétale ou animale par une légère altération.

L'amidine obtenue par l'évaporation de sa dissolution aqueuse, se présente, suivant le mode du desséchement, en fragments blancs, opaques, et irréguliers, ou sous l'apparence d'une matière jaune pâle, à demi transparente (comme de la gomme arabique), et très friable. Elle est insoluble dans l'alcool. L'eau froide mise en macération sur l'amidine en dissout environ $\frac{1}{10}$ème de son poids, et présente après sa filtration une liqueur sans couleur, et très fluide.

L'eau dissout l'amidine en toute proportion à une température d'environ 50° R., et elle en retient en dissolution, après

son refroidissement, une beaucoup plus grande proportion que celle dont se charge ce liquide lorsqu'il agit à froid sur cette substance. La décoction peut être rapprochée par l'évaporation au point de contenir le quart de son poids d'amidine en dissolution, sans se troubler ou sans se convertir en pâte et en gelée par le refroidissement ; ce qui n'a point lieu pour l'amidon. Lorsque la dissolution d'amidine est plus rapprochée, elle se précipite en partie, par le refroidissement, en une matière blanche et opaque ; mais cette dernière se dissout, en présentant une liqueur transparente, à une temperature de 50° R. : sous ce rapport elle se rapproche de l'inuline. La dissolution aqueuse d'amidine faite à froid, et qui en contient la dixième de son poids, se colore en bleu avec la solution aqueuse d'iode, et présente avec ce réactif tous les effets de l'amidon.*

La même dissolution est coagulée en une pâte blanche et opaque par le sous-acétate de plomb : l'acétate neutre n'y fait qu'un précipité peu sensible.

Elle est abondamment troublée par l'eau de baryte, et point par l'eau de chaux. La décoction de noix de gale n'y produit pas de changement bien marqué.

Les solutions aqueuses de potasse dissolvent l'amidine. Ces combinaisons sont très fluides, et ne se présentent point dans l'état visqueux et filant de celles d'amidon. Les acides foibles en précipitent l'amidine avec toutes ses propriétés.

L'alcool y produit aussi un précipité abondant, mais ce dernier retient une certaine proportion d'alcali qui fait que

* Annales de Chimie, T. 90. Mémoire sur l'iode par MM. Colin et Gaultier de Claubry.

l'amidine précipité ne se colore en bleu par l'iode, que lorsqu'on y ajoute un acide.

L'amidine diffère donc principalement de l'amidon, en ce que l'eau froide peut la dissoudre, en ce qu'elle ne forme point de gelée avec l'eau bouillante, ni des combinaisons visqueuses avec les lessives de potasse. Les caractères qui la distinguent du principe gommeux dont j'ai parlé précédemment, sont, 1°, de n'être pas soluble dans l'eau froide en toute proportion; 2°, de colorer en bleu la solution aqueuse d'iode; 3°, de former avec l'eau une dissolution qui est coagulée par le sous acétate de plomb.

Je n'ai point pu obtenir d'amidine par la germination du froment. Si ce produit s'y forme, c'est sans doute en trop petite quantité pour qu'on puisse le séparer de l'amidon. Dans l'analyse que j'ai faite de 100 parties de froment, avant, et après sa germination, 6 parties de sa substance farineuse ont paru détruites par cette végétation, et elles ont été remplacées par 3½ parties de mucilage, et 2½ de sucre sec déliquescent. Si, comme on peut le présumer, l'amidine se forme en aussi petite quantité que ces produits, elle peut rester confondue avec l'amidon sans qu'on puisse l'en séparer. D'ailleurs je n'ai pas trouvé, malgré les assertions contraires de MM. Proust et Döbereiner, que l'amidon du froment fut notablement changé dans ses propriétés par la germination.

Note C, *sur le changement de poids qu'éprouve l'amidon par sa fermentation à l'air*, § II. § X. § XIII.

En analysant le sucre produit par l'action de l'acide sulfurique et de l'eau sur l'amidon (Bibliothèque Brit. Sc. et Arts. v. 56) j'ai trouvé que cette production s'opère par la fixation

des éléments de l'eau dans l'amidon. Je suis parvenu à la même explication, on plutôt à sa confirmation en montrant que la quantité de sucre formée par ce procédé est plus grande que celle de l'amidon employé, lorsqu'ils sont séchés l'un et l'autre à la température de l'eau bouillante. Cette observation pourroit paroitre opposée à celle qui montre que le résidu de la décomposition de l'amidon à l'air pèse moins que l'amidon employé: mais il est peut être superflu d'observer que ces effets ne sauroient être comparés; parce que dans celui de l'acide sulfurique, le sucre est en dernier résultat le seul produit notable de l'opération; tandis que dans la fermentation de l'amidon à l'air, il se forme plusieurs produits dans lesquels les éléments de l'eau sont répartis d'une manière très inégale, et se perdent en partie par l'évaporation.

Par la fixation des éléments de l'eau dans l'amidon pour former le sucre, je n'entends pas qu'ils s'y trouvent dans l'état d'eau solidifiée ou d'eau de crystallisation; la manière dont ils sont répartis est encore indéterminée.

Cette fixation de l'eau a lieu sans doute plus souvent qu'on ne le pense dans le traitement des substances végétales ou animales par les procédés ordinaires de nos laboratoires. J'ai trouvé que les nouvelles propriétés que les graisses acquièrent par la saponification, tiennent principalement à la fixation des éléments de l'eau dans la graisse.

100 parties de graisse de porc lavée et filtrée, m'ont fourni par leur combustion dans du gaz oxigène,*

* Cette analyse ne s'accorde pas avec celle que Berard a faite de la même substance en la traitant par l'oxide de cuivre. Ses résultats ainsi qu'un grand nombre de ceux obtenus par ce procédé, m'ont paru pécher par excès d'hydrogène. (Annales de Ch. et de Phys. T. V.)

Carbone - -	78,848.
Hydrogène - -	12,182.
Oxigène - -	8,502.
Azote - - -	0,478.
	100

100 parties de la même graisse saponifiée ou précipitée de son savon de potasse par l'acide muriatique contiennent,

Carbone - -	75,747.
Hydrogène - -	11,615.
Oxigène - -	12,825.
Azote - -	0,813.
	100

Le gaz oxigène consumé et le gaz acide carbonique produit par la combustion de la graisse, avant et après sa saponification, sont presque dans le même rapport, savoir comme 100 : 71,5 dans la 1ère analyse; et comme 100 : 72,7 dans la 2de. Il en résulte qu'on pourroit presque considérer ces deux espéces de graisse comme ayant la même base unie a différentes quantités d'eau, quoique ce ne soit point là probablement le véritable mode de leur composition.

Les expériences de Chevreul que j'ai vérifiées ne sont pas opposées à cette explication en indiquant qu'il y a augmentation de poids dans le produit de l'opération. 100 parties de graisse de porc fournissent suivant ce chimiste, 94 ou 95 de graisse saponifiée, et environ 9 de principe doux sans compter la perte qui a lieu dans la manipulation : ces produits paroissent donc surpasser le poids de la graisse qui les a fournis.

Note D, sur le gaz hydrogène produit par la fermentation de l'amidon, § XIII.

On peut être surpris que le gaz hydrogène formé par la décomposition spontanée de l'empois d'amidon, soit du gaz hydrogène pur, et non point un gaz hydrogène carburé semblable à celui des marais : mais toutes les substances végétales que j'ai fait fermenter par un procédé analogue à celui qui a été employé pour l'empois, ont dégagé du gaz hydrogène pur ou presque pur, abstraction faite du gaz acide carbonique qui y étoit mêlé, et de la faculté qu'avoient quelques unes d'entr'elles de ne dégager que ce dernier.

2,85 grammes de gluten frais qui contenoit la 0,37 partie de son poids d'eau, et au quel j'ai ajouté 16 grammes de ce liquide, ont dégagé dans cinq semaines, après avoir été placés sous un récipient renversé plein de mercure, 80 centim. cubes de gaz, sans y comprendre celui que l'eau a retenu. Les 80 étoient composés de 60 de gaz acide carbonique et de 20 de gaz hydrogène qui n'a pas formé une quantité notable de gaz acide carbonique par sa combustion.

Le froment dans une fermentation semblable n'a dégagé que du gaz acide carbonique, sans mélange de gaz hydrogène.

8 grammes de graines de pois sèches aux quelles j'ai ajouté 12 grammes d'eau, ont dégagé dans trois semaines, par un procédé semblable, 117 centimètres cubes de gaz qui contenoit 88 de gaz acide carbonique et 29 de gaz hydrogène.

J'ai répété ces expériences sur une livre de pois, en les enfermant sous un recipient plein d'eau, renversé sur ce liquide en contact avec l'air. La pesanteur spécifique du gaz

hydrogène dégagé, abstraction faite du gaz azote qui le souilloit, étoit 0,0883. 100 parties de cet air inflammable que j'ai pu analyser plus en grand que les précédents, ont consumé 55,45 de gaz oxigène, en formant 2,64 de gaz acide carbonique.

Ces expériences ont été faites avec quelques autres graines; et celles qui ont produit du gaz hydrogène l'ont toujours dégagé pur ou presque pur, mais mêlé avec du gaz acide carbonique dans une proportion qui n'a souffert que peu de variations, c'est-à-dire environ dans le rapport de 1 : 4 en y comprenant le gaz acide que l'eau a retenu; tel étoit aussi le mélange de gaz que l'amidon et le gluten ont fourni.

La formation d'un gaz hydrogène aussi pur par la fermentation n'avoit pas été observée: elle conduit à chercher la raison pour la quelle la fermentation des marais produit un gaz inflammable aussi différent du précédent.

IV. *On Corpora Lutea.* *By Sir* Everard Home, *Bart. V. P. R. S.*

Read January 14, 1819.

In May, 1817, I laid before the Society an account of the human ovum, and not only showed the cavity of the corpus luteum from which it had escaped, but another corpus luteum in the same ovarium, which had made a considerable advance in its growth.

Ever since that time I have been actively employed, with the assistance of Mr. Bauer, in tracing the rise and progress of the corpus luteum to its full growth, its use, and afterwards its decay. The result of our labours is contained in the present communication, and in the drawings which accompany it.

Corpora lutea are never met with before puberty; the natural structure of the ovarium is therefore more readily ascertained before that period. It is nearly the same in different animals; is of a loose open texture, in which, more particularly near the circumference, a number of small cells of a globular form are met with. This structure is shown in the annexed drawings, in the human ovarium, [Plate III. fig. 2.] in that of the cow, [Plate VI. fig. 2.] and in that of the sow, [Plate VIII. fig. 2.]

The corpus luteum, from its first appearance, seems to be an entirely new substance, distinct from that of the ovarium itself; it is never formed within the cells, but in the substance of the ovarium; [See Plate IV. fig. 2 and 4: Plate VI. fig. 4: and Plate VIII. fig. 4:] and compresses the surrounding

parts so much, that when full grown, and even when there are several in the same ovarium, that body is not much encreased beyond its natural size.

The structure of the corpus luteum is of a very particular kind, and is not distinctly seen in small animals, or in those that have numerous litters; but in the cow, which has commonly only one calf at a birth, the corpus luteum is so large, that when it is magnified, the structure can be made out; it is a mass of thin convolutions, bearing a greater resemblance to those of the brain than of any other organ. Its form is an irregular oval with a central cavity, and in some animals its substance is of a bright orange colour when first exposed; all these appearances are most accurately displayed in the annexed drawings of it in the cow. [Plate VII. fig. 2, 3.] Corpora lutea are found to make their appearance in the ovaria at the age of puberty, and continue to succeed each other as the young are produced, till the period arrives when breeding no longer goes on. [Plate III. fig. 7; Plate IV. fig. 6; Plate IX. fig. 6.]

As the object of the present paper is to draw conclusions from the appearances that are represented in the annexed drawings, in proof of corpora lutea being the structures in which the ova are formed; of their being produced previous to, and independent of sexual intercourse; and when they have fulfilled their office of forming ova, being afterwards removed by absorption, whether the ova are impregnated or not; I shall not take up the time of the Society longer than in detailing a number of facts, which indeed will be doing little more than giving a catalogue raisonnée of Mr. BAUER's drawings, which put these facts upon record. That corpora

lutea are formed in a state of virginity, is proved both in the human species and the hog tribe, as will be seen in the annexed drawings. In a young woman of twenty years of age with a perfect hymen, one of the ovaria was found by Mr. BAUER to contain a corpus luteum, in the cavity of which there was an ovum which had nearly arrived at its full size; the second covering or chorion had already formed, by means of which the ovum had a slight adhesion to the inner surface of the corpus luteum. [Plate III. fig. 4.] When this ovum was examined after it had been removed from its situation, [Plate III. fig. 5.] its figure was the same as that found in the uterus and described in my former paper; it only differed in being smaller in the proportion of $\frac{1}{200}$ to $\frac{1}{100}$, in the whole being transparent, and in the chorion not having extended itself completely over the anterior surface of the ovum. The Fallopian tube on that side was fuller than the opposite. The fimbriæ were spread out, and unusually vascular; so that every preparation was made for the reception of the ovum into the tube. No sexual intercourse had taken place. [Plate III. fig. 3.]

I have met with corpora lutea in virgins at 14; and know of two instances of girls having children still earlier, one at 13, the other at 12.

Sir JOHN SEBRIGHT, whose knowledge respecting pigeons is well known, informs me, that when mated, they lay eggs earlier than when kept from the male; they do not lay a greater number of eggs, but they lay them at all seasons, while the others lay only in the spring.

In the cow, the age of puberty is considered to be two

years old, and the corpus luteum represented in the engraving is that of a first calf. [Plate VI. fig. 4; and Plate VII. fig. 2, and 3.] In the hog tribe, the age of puberty is six months; and in the series of drawings of their ovaria, one is given from a sow pig at four months, in which no corpora lutea had begun to appear. [Plate VIII. fig. 2.] In another, between five and six months, several have made a considerable advance, [Plate VIII. fig. 4.] and in one nearly six months they were completely formed, and their cavities filled with blood, similar to the human corpus luteum containing the ovum, but no ova were detected. [Plate VIII. fig. 6.]

In another virgin pig, nearly six months old, Mr. BAUER was so fortunate as to meet with corpora lutea in the very act of bursting to part with their ova: the appearance is shown in Plate IX. fig. 1 and 2. From this most fortunate occurrence we learn that animals part with their eggs whether there is sexual intercourse or not, and this is done with such force, that the cavity of the corpus luteum is absolutely inverted, so that the ovum is exposed completely to the emission of the male. The extravasation of blood in rupturing the ovarium, and inverting the corpus luteum, is in many instances so great, that some of it passes out through the vagina, which when met with, is considered a sign of impregnation having taken place. As soon as the ovum is expelled, the corpus luteum recovers itself, and returns to its natural state. When the ovum accidentally adheres by a portion of the chorion to the inner surface of the corpus luteum, as is shown to be the case in Plate III. fig. 4, it may retain its hold; and however compleat its expulsion, may be carried back, and form what is

called an ovarium case, the fœtus growing in the ovarium; several cases of this kind are met with, particularly one now in the collection of the Emperor of RUSSIA, purchased from the late Mr. CRUIKSHANK, teacher of Anatomy in London.

An instance of corpora lutea forming in succession, and probably to the same number, whether the female is ever impregnated or not, is shown in a drawing of the ovarium of a woman who died a virgin at 47 years old; the fragments of seven corpora lutea are distinctly seen in it, and in the opposite there were five, putting on the same appearance. [Plate III. fig. 7.]

After the escape of the ova, the corpora lutea have their cavities distended with blood, which coagulates, loses its colour, and forms a white solid mass surrounded by broken portions of the corpus luteum. [Plate IV. fig. 4 and 5.] These become smaller and smaller, till they disappear. A series of them is shown in the human ovarium, six weeks and nine months after impregnation; also after a woman had borne 12 children, and had for many years left off breeding. [Plate V. fig. 6.]

The remains of the corpus luteum, at nine months after impregnation of the ovum, are so indistinct as hardly to be recognized; but in the opposite ovarium there is commonly a corpus luteum far advanced, forming another ovum; and it will be found that all the preparations of corpora lutea taken from the ovaria of women who die in child-bed, actually belong to this new ovum not yet completely formed. [Plate V. fig. 2, and fig. 4.]

In some cases, the coagulum filling the cavity of the corpus luteum is absorbed, leaving a circular cup, whose margin is

fringed with small portions of the substance of the corpus luteum; this has been usually taken for a perfect corpus luteum, and preserved as such.

The cells met with in the ovaria before puberty are globular; but as the ovaria increase in size, the sides of these cells become squeezed, which gives them an oval form. [Plate VI. fig. 4. and Plate VII. fig. 2 and 3.]

EXPLANATION OF THE PLATES.

PLATE III.

In this Plate are represented six figures of the human ovarium in a virgin state, and one of the human ovum before it is impregnated. The external appearances of the ovaria are of the natural size. The internal appearances are magnified two diameters, the ovum itself is magnified twenty-two diameters.

Fig. 1. the human ovarium at twelve years of age, showing its external form.

Fig. 2. An internal view of the same: the blood vessels are injected, its structure is more compact than natural, the parts having been preserved in spirit. The cavities contained coagulated lymph and a serous fluid.

Fig. 3. The external view of the ovarium at 20 years of age, after having been some days in spirit.

Fig. 4. An internal view of the same, showing that although the woman was a virgin, there was a corpus luteum arrived at its full growth containing an ovum, which has two membranous coverings, the amnion and chorion; the cavity surrounding the ovum was filled with blood; and the

ovum, by means of the chorion, has a slight attachment to the inner surface of the corpus luteum.

Fig. 5, represents the ovum in a detached state; it is smaller than that represented in my former paper, which had arrived at the cavity of the uterus, in the proportion of $\frac{1}{200}$ to $\frac{1}{100}$, and differs from it in being pellucid, the two opaque spots met with in the other not yet being formed.

Fig. 6. The external form of the ovarium at 47 years of age.

Fig 7. An internal view of the same, showing the remains of seven corpora lutea. The central cavities are distended with coagulated blood, as in all the corpora lutea met with after the ova that escaped from them had been impregnated. The corresponding ovarium contained five corpora lutea, similar in appearance to those here represented.

PLATE IV.

Contains five figures of the human ovarium after impregnation, the external appearances are of the natural size, the internal ones are magnified two diameters.

Fig. 1. The external appearance of the ovarium represented in a former paper on the human ovum, and shown again here to complete the series.

Fig. 2. An internal view of the same, to show the appearance of the corpus luteum five days after the impregnation of the ovum.

Fig. 3. The external appearance of the ovarium, six weeks after the impregnation of the ovum, which was arrested in the Fallopian tube.

Fig. 4. An internal view of the same, showing the corpus luteum partly absorbed, and the remaining parts unconnected with each other.

Fig. 5. A transverse section of the same ovarium, and its corpus luteum.

PLATE V.

Contains six figures of the human ovarium at different periods after impregnation, all of them of the natural size, except Fig. 6, which is magnified two diameters.

Fig. 1. An external view of the ovarium, that did not contain the ovum from which the child was produced, taken immediately after the child was born.

Fig. 2. An internal view of the same, in which there is a corpus luteum nearly arrived at its full size.

Fig. 3. The external view of the ovarium, in which the impregnated ovum had been formed.

Fig. 4. An internal view of the same, showing how much the corpus luteum had been broken down, and the want of distinctness in the remaining parts. There is also a new corpus luteum forming.

When Fig. 2 and Fig. 4 are compared, it will be seen, that all corpora lutea which have been preserved after the mother dies in child-birth, do not belong to the ovum of the child born, but to that which is to succeed it.

Fig. 5. The external appearance of the ovarium of a woman who had twelve children, and died at seventy years of age.

Fig. 6. Internal view of the same, to show how small the remains of the corpora lutea had become at that age. The large cyst contained coagulated lymph and serum.

PLATE VI.

Contains four figures of the ovaria of the cow. The external appearances of the natural size, the internal magnified two diameters.

Fig. 1. The ovarium of a calf, two months old.

Fig. 2. An internal view of the same.

Fig. 3. External view of the unimpregnated ovarium of a cow, 14 days after her being with calf.

Fig. 4. Internal view of the same, representing the commencement of a corpus luteum, and showing that the cells in the ovarium which before puberty are met with in a globular form, become gradually of an oval and irregular shape as the ovarium increases in size.

PLATE VII.

Contains three figures of the ovarium of a cow with calf. The external appearance is of the natural size; and the internal is magnified two diameters.

Fig. 1. External view of the corresponding ovarium.

Fig. 2. Internal view of the same, showing the structure of the corpus luteum, which in this animal is very large, and is evidently made up of convolutions more nearly resembling those of the brain, than of any other organ.

Fig. 3. A transverse section of the corpus luteum,

PLATE VIII.

Contains six figures of the ovaria of the pig, in a virgin state. The external appearances of the natural size, the internal magnified two diameters.

Fig. 1. The external view of the ovarium at four months.

Fig. 2. Internal view of the same.

Fig. 3. External view of the ovarium at nearly six months.

Fig. 4. Internal view of the same, showing three incipient corpora lutea.

Fig. 5. External view at six months.

Fig. 6. Internal view, showing six corpora lutea, nearly if not quite at the full size: the cavities filled with blood, as in Plate III. fig. 4, but no ova were detected.

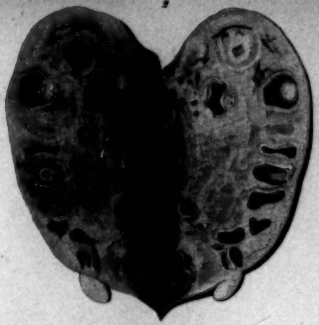
PLATE IX.

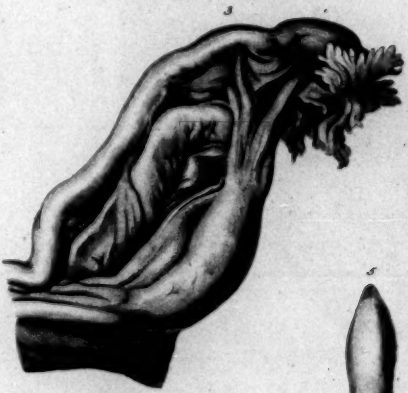
Contains six figures of ovaria of the pig; two in a virgin state, nearly six months old, in which the corpora lutea are in the act of bursting, and four of the sow after impregnation.

Fig. 1. The external view of the ovarium, showing five inverted corpora lutea, spreading over the surface of the ovarium, considerably beyond the orifices through which they protruded; their inner surface is turned inside out, by which means the ova must have been completely discharged. No ova were detected.

Fig. 2. Internal view of the same, showing four corpora lutea, and a great number of blood-vessels, which at that instant become very visible. Both figures are magnified two diameters.

Fig. 1.





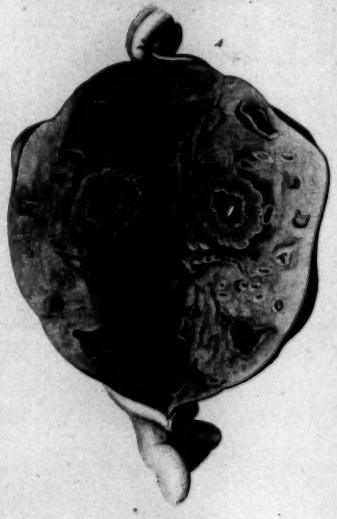
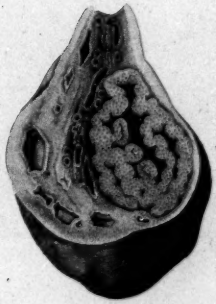


Fig. 1.



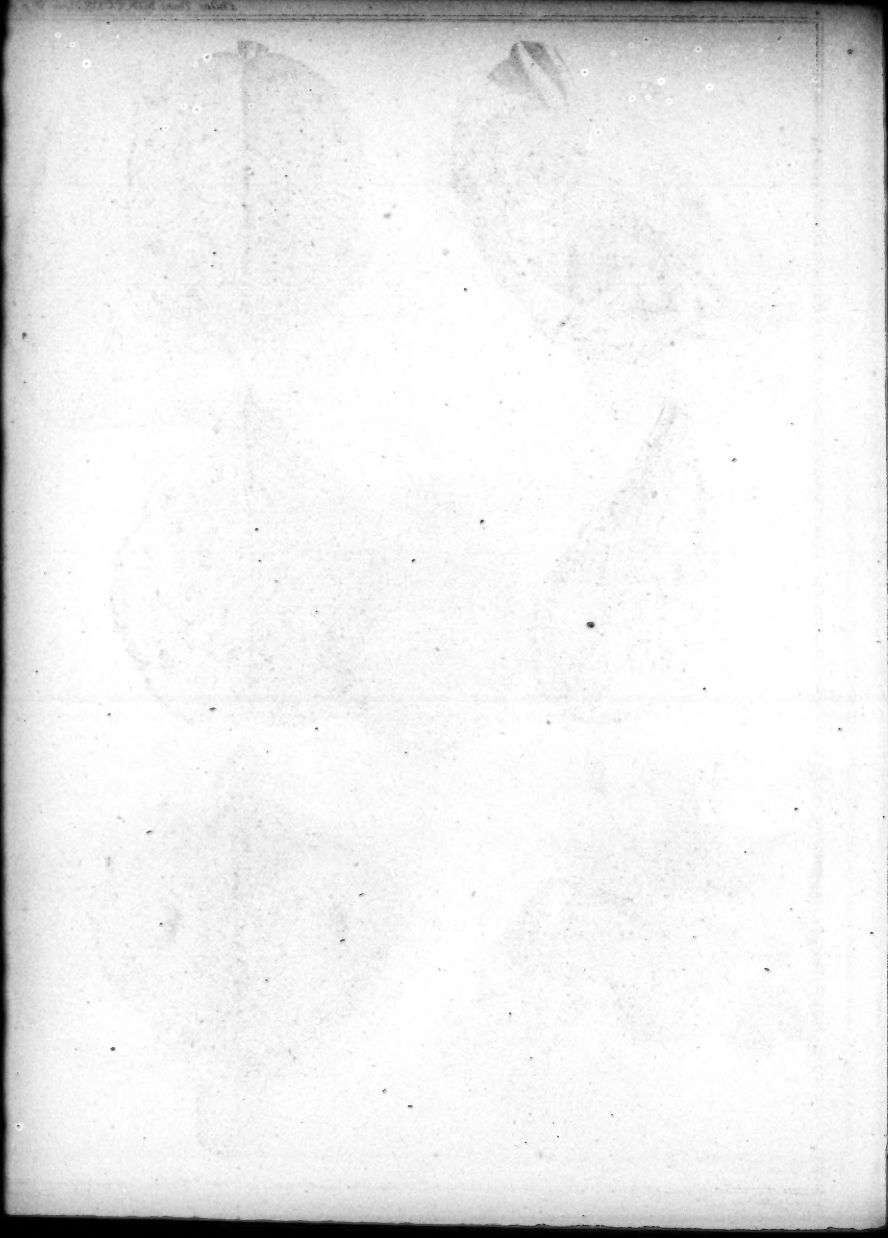


Fig. 1.

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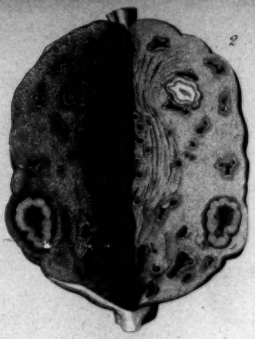

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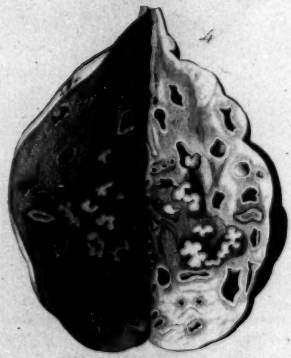

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Fig. 1.

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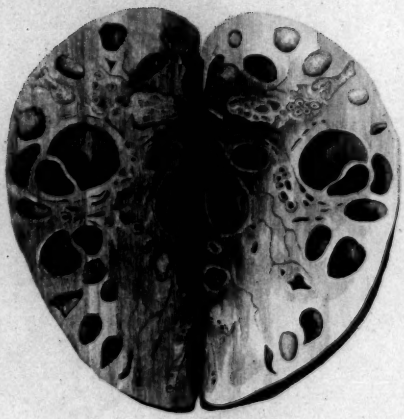

Fig. 1.

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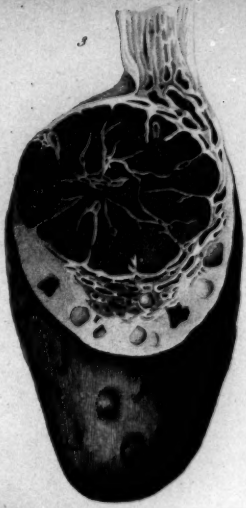


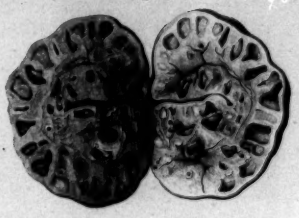
2

Franz Bauer, del.

J. Basire, sc.

Fig. 1.

2



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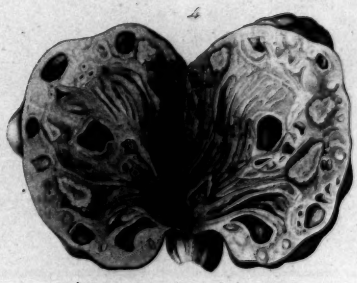

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6

Franz Dauer. del.

J. Bauer. sc.

Fig. 1.

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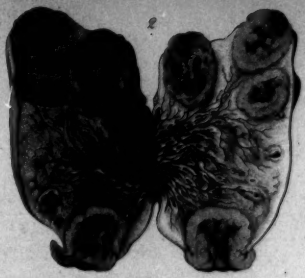
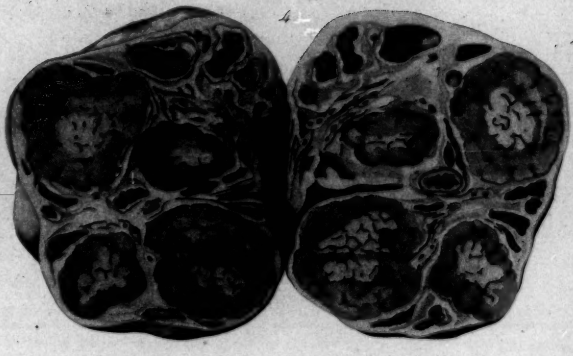


3

Natural Size.

4.



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Natural Size.

6.

Fig. 3. An external view of the ovarium of a sow, 14 days after impregnation.

Fig. 4. The internal view, in which there are five corpora lutea, their appearance becoming very indistinct.

Fig. 5. The ovarium of a sow five years old, that had in all six litters of pigs, and had left off breeding for a year.

Fig. 6. An internal view of the same, to show that all vestiges of corpora lutea were nearly removed.

Since the first part of this Paper was printed, a fœtus of the size usually met with at the end of the fourth month, has been found compleatly inclosed in the ovarium. It is evident that the ovum, after impregnation, was retained in the cavity of the corpus luteum in the manner explained above, since vestiges of the corpus luteum are still visible between the chorion and the substance of the ovarium. The mother died in consequence of hæmorrhage, produced by the bursting of a branch of the spermatic artery. An account of the case, with drawings of the parts made by Mr. BAUER, will be laid before the Society, after the long vacation, by Dr. GRANVILLE.

V. *Remarks on the probabilities of error in physical observations, and on the density of the earth, considered, especially with regard to the reduction of experiments on the pendulum. In a letter to Capt.* HENRY KATER, *F. R. S. By* THOMAS YOUNG, *M. D. For. Sec. R. S.*

Read January 21, 1819.

MY DEAR SIR,

THE results of some of your late experiments on the pendulum having led me to reflect on the possible inequalities in the arrangement of gravitating matter within the earth's substance, as well as on the methods of appreciating the accuracy of a long series of observations in general, I have thought that it might be agreeable to you, to receive the conclusions which I have obtained from my investigations, in such a form as might serve either to accompany the report of your operations, or to be laid before the Royal Society as a distinct communication.

1. *On the estimation of the advantage of multiplied observations.*

It has been a favourite object of research and speculation, among the authors of the most modern refinements of mathematical analysis, to determine the laws, by which the probability of occurrences, and the accuracy of experimental results, may be reduced to a numerical form. It is indeed true, that this calculation has sometimes vainly endeavoured to substitute arithmetic for common sense, and at other times has exhibited an inclination to employ the doctrine of chances as a sort of auxiliary in the pursuit of a political object, not

otherwise so easily attainable; but we must recollect, that at least as much good sense is required in applying our mathematics to objects of a moral nature, as would be sufficient to enable us to judge of all their relations without any mathematics at all: and that a wise government and a brave people may rely with much more confidence on the permanent sources of their prosperity, than the most expert calculators have any right to repose in the most ingenious combinations of accidental causes.

It is however an important, as well as an interesting study, to inquire in what manner the apparent constancy of many general results, which are obviously subject to great and numerous causes of diversity, may best be explained: and we shall soon discover that the combination of a multitude of independent sources of error, each liable to incessant fluctuation, has a natural tendency, derived from their multiplicity and independence, to diminish the aggregate variation of their joint effect; and that this consideration is sufficient to illustrate the occurrence, for example, of almost an equal number of dead letters every year in a general post office, and many other similar circumstances, which, to an unprepared mind, seem to wear the appearance of a kind of mysterious fatality, and which have sometimes been considered, even by those who have investigated the subject with more attention, as implying something approaching more nearly to constancy in the original causes of the events, than there is any just reason for inferring from them.

This statement may be rendered more intelligible by the simple case of supposing an equal large number of black and white balls to be thrown into a box, and 100 of them to be

drawn out either at once or in succession. It may then be demonstrated, as will appear hereafter, from the number of ways in which the respective numbers of each kind of balls may happen to be drawn, that there is 1 chance in $12\frac{1}{2}$ that exactly 50 of each kind may be drawn, and an even chance that there will not be more than 58 of either, though it still remains barely possible that even 100 black balls or 100 white may be drawn in succession.

From a similar consideration of the number of combinations affording a given error, it will be easy to obtain the probable error of the mean of a number of observations of any kind; beginning first with the simple supposition of the certainty of an error of constant magnitude, but equally likely to fall on either side of the truth, and then deducing from this supposition the result of the more ordinary case of the greater probability of small errors than of larger ones. This liability to a constant error may be represented, by supposing a counter to have two faces, marked 0 and 2; the mean value of an infinite number of trials will then obviously be 1, and the constant error of each trial will be 1, whether positive or negative.

Now in a combination of n trials with such a counter, if we divide the sum of the results by n, the greatest possible error of the mean thus found will be 1; and the probability of any other given error will be expressed by the number of combination of the faces of n counters affording that error, divided by the whole number of combinations; that is, by the corresponding coefficient of the binomial $(1+1)^n$, divided by 2^n, the sum of the coefficients. The calculation therefore will stand thus:

	$n=2$	$n=3$	$n=4$	$n=6$	$n=8$
Coefficients	1 2 1	1 3 3 1	1 4 6 4 1	1 6 15 20 ...	1 8 28 56 70 ...
Numbers thrown	0 2 4	0 2 4 6	0 2 4 6 8	0 2 4 6 ...	0 2 4 6 8 ...
Differences from n	2 0 2	3 1 1 3	4 2 0 2 4	6 4 2 0 ...	8 6 4 2 0 ...
Errors of the means	1 0 1	1 $\frac{1}{3}$ $\frac{1}{3}$ 1	1 $\frac{1}{2}$ 0 $\frac{1}{2}$ 1	1 $\frac{2}{3}$ $\frac{1}{3}$ 0 ...	1 $\frac{3}{4}$ $\frac{1}{2}$ $\frac{1}{4}$ 0 ...
Sums of errors	$1+0+1=2$	$1+1+1+1=4$	$1+2+0+2+1=6$	$1+4+5+0$ ...	$1+6+14+14+0$ ...
Mean errors	$\frac{2}{2}=\frac{1}{1}$	$\frac{4}{3}=\frac{1}{1}$	$\frac{6}{8}=\frac{3}{4}$	$\frac{10}{24}=\frac{5}{12}$	$\frac{35}{128}=\frac{35}{128}$

It is easy to perceive that these coefficients must express the true numbers of the combinations, since they are formed by adding together the two adjacent members of the preceding series; thus when n is 3, 1 combination giving the number 0 and 3 the number 2, these two combinations, being again respectively combined with 2 and 0 of a fourth counter, give $1+3=4$, for the combinations affording the number 2 in the next series; while each succeeding series must continue to begin and end with unity, since there is only one combination that can afford either of the extremes.

In order to continue the calculation with greater convenience, we must find a general expression for the middle terms, 2, 6, 20, 70..., neglecting the odd values of n. The first, 2, is made up of $(1+1)$, the second, 6, is $2\,(2+1)$; 20 is $2\,(6+4)$ and $70=2\,(20+15)$: or $6=2\,(2.\frac{3}{2})$, $20=2\,(6.\frac{4}{3})$, $70=2\,(20.\frac{7}{4})$, whence the series may easily be continued at pleasure, multiplying always the preceding term by $\frac{4}{2}$, $\frac{10}{3}$, $\frac{14}{4}$, $\frac{18}{5}$,... We have also $6=16.\frac{3}{8}=2^4.\frac{3}{8}$, $20=2^4.\frac{5}{16}$, and $70=2^3.\frac{35}{128}$: consequently the terms of this series, divided by 2^n, will always express the mean errors already calculated. From this value of the middle term we may easily deduce that of the neighbouring terms by means of the original formula $n.\frac{n-1}{2}..\frac{\frac{1}{2}n+1}{\frac{1}{2}n}.\frac{\frac{1}{2}n}{\frac{1}{2}n+1}.\frac{\frac{1}{2}n-1}{\frac{1}{2}n+2}$...; the

first factor less than unity being always $\frac{\frac{1}{2}n}{\frac{1}{2}n+1} = \frac{1}{1+\frac{2}{n}}$. The magnitude of the mean error is exhibited in the annexed table.

n	Mean error
2	.500000
4	.375000
6	.312500
8	.273437
10	.246094
12	.225586
14	.209473
16	.196381
18	.185471
20	.176196
30	.144466
40	.125363
50	.112271
60	.102574
70	.095022
80	.088924
90	.083868
100	.079586

The general expression for this series being $\frac{1}{2} \cdot \frac{3}{4} \cdot \frac{5}{6} \ldots \frac{n-1}{n}$, it is obvious that if we multiply it by $\frac{2}{3} \cdot \frac{4}{5} \cdot \frac{n-2}{n-1}$, the product will be $\frac{1}{n}$, whatever the value of n may be: and when that value is large, the factors of these two expressions will approach so near to each other that they may be considered as equal; consequently the corresponding terms of either, taken between any two large values of n, will vary in the subduplicate ratio of n, since their product, which may be considered as the square of either, varies in the simple ratio of n, so that the mean error may ultimately be expressed by $\sqrt{\frac{1}{pn}}$. The value of p evidently approximates to that of the quadrant of a circle, of which the radius is unity: thus for $n = 10$ it is 1.6512, and for $n = 100$, 1.5788, instead of 1.5708; and the ultimate identity of these magnitudes has been demonstrated by EULER and others. (See Mr. HERSCHEL's Treatise on Series, in LACROIX, Engl. Ed. n. 410.)

The fraction thus found, multiplied by 2^n, gives the number of combinations expressed by the middle term, in which the error vanishes, when n is even: and the whole number of

combinations being also 2^n, it is obvious that the fraction alone must express the probability of a result totally free from error. The neighbouring terms on each side, for $n = 100$, are .078025, .078524, and .066588, the sum of the 7 being .515860; and since this sum exceeds $\frac{1}{2}$, it is obviously more probable that the result of 100 trials will be found in some of these seven terms, than in any of the remaining 94, and that the mean error will not exceed $\frac{3}{100}$. When n is so large, that the terms concerned may be considered as nearly equal, the factors $\frac{\frac{1}{2}n}{\frac{1}{2}n+1}$, $\frac{\frac{1}{2}n-1}{\frac{1}{2}n+2}$..., may be expressed by $1-\frac{2}{n}$, $1-\frac{6}{n}$, $1-\frac{10}{n}$..., and the terms themselves by 1, $1-\frac{2}{n}$, $1-\frac{8}{n}$, $1-\frac{18}{n}$... the negative parts forming the series $\frac{2}{n}(1, 4, 9\ldots)$ of which the sum, for q terms, is $\frac{2}{n}(\frac{1}{3}q^3+\frac{1}{2}q^2+\frac{1}{6}q)$ or ultimately $\frac{2}{3n}q^3$; consequently if we call the middle term e, we must determine q in such a manner as to have $e(2q-\frac{4}{3n}q^3)=\frac{1}{2}-e$, and $q(1-\frac{2}{3n}q^2)=\frac{1}{4e}-\frac{1}{2}$; but e has been already found, in this case, $=\sqrt{\frac{1}{pn}}$, and neglecting at first the square of q, we have $q=\frac{1}{4}\sqrt{(pn)}-\frac{1}{2}$, and $q^2=\frac{1}{16}pn$, whence $\frac{2}{3n}q^2=\frac{1}{24}p$, and $1-\frac{2}{3n}q^2=.93455$; hence, for a second approximation, $.93455\,q=\frac{1}{4e}-\frac{1}{2}$, and $q=.2674\sqrt{(pn)}-.53$; and by continuing the operation we obtain $.9285\,q=\frac{1}{4e}-\frac{1}{2}$, and $q=.271\sqrt{(pn)}-.54$; consequently the probable error, being expressed by $\frac{2q}{n}$, will be $.542\sqrt{\frac{p}{n}}-\frac{1.08}{n}=\frac{.679}{\sqrt{n}}-\frac{1.08}{n}$. This formula, for $n=100$, becomes .0571, and for $n=10000$, .00679 — .00011 = .00668.

We must not, however, lose sight, in this calculation, of the original condition of liability to a certain constant error in each trial. For example, we may infer from it, that if we made 100 observations of the place of a luminary, each differing 1′ from the truth, but indifferently on either side of it, the error of the mean result would probably not exceed $\frac{3}{50}$.1′ = 8.6″; and that in 1000 observations it would probably be reduced to about a second. Now although, in the methods of observing which we employ, the error is liable to considerable variations, yet it may be represented with sufficient accuracy, by the combination of two or more experiments in which the simpler law prevails. For example, the combination of two counters, such as have been considered, is equivalent to the effect of a die with four faces, or a tetraedron, marked 0, 2, 2, and 4, or with errors expressed by 1, 0, 0, and −1; the combination of three counters is represented by a die having eight faces, or an octaedron, with the errors 1, $\frac{1}{3}$, $\frac{1}{3}$, $\frac{1}{3}$, $-\frac{1}{3}$, $-\frac{1}{3}$ $-\frac{1}{3}$, −1; and the combination of four, by a solid of 16 sides, with the errors 1, $4 \times \frac{1}{2}$, 6×0, $4 \times -\frac{1}{2}$, −1. These distributions evidently resemble those which are generally found to take place in the results of our experiments; and it is of the less consequence to represent them with greater accuracy, since the minute steps, by which the scale of error varies, have no sensible effect on the result, especially when the number of observations is considerable. If, for example, instead of two trials with the tetraedron, having the errors 1, 0, 0, −1, we made two trials with a solid of 21 faces, having the errors distributed equally from 1, .9, .8 .. to −1, the mean error of all the possible combinations would only

vary from .575 to .849; and in a greater number of trials the errors would approach still nearer to equality.

Now in order to employ any of these suppositions for the purpose of calculation, it is only necessary to compute the corresponding mean error, and to make it equal to the actual mean error of a great number of observations. Thus, if we consider each observation as representing a binary combination of counters or constant errors, in which the mean error is $\frac{1}{2}$, and adding together the differences of the several results from the mean, and dividing by their numbers, we find the mean error of 100 observations 1′, we must consider the original constant error as equal to 2′, which is to be made the unit for 100 primitive combinations; and $\frac{.679}{\sqrt{100}} - \frac{1.08}{200} = .0426$; and the probable error of the mean will be $.0426 \times 120 = 5.1''$. For a quaternary combination, if the error, which amounts to $\frac{3}{8}$, be found 1′, the unit will be $\frac{4}{3}'$, and for $n = 400$, we have $.03125 \times \frac{4}{3}' = 5.0''$. And if we set out with a large number m of combinations, the mean error being $\sqrt{\frac{1}{pm}} = e$, the unit will be $e\sqrt{(pm)} = 1$, and the probable error of nm trials being equal to this unit multiplied by $.542\sqrt{\frac{p}{nm}}$, neglecting the very small fraction $\frac{1.08}{nm}$, we have $.542\sqrt{\frac{p}{nm}}\, e\sqrt{(pm)} = .542\, p\sqrt{\frac{1}{n}}\, e = .8514\sqrt{\frac{1}{n}}\, e$: which, if e be 1′, and $n = 100$, gives again $5.1''$. It appears therefore that the supposition, respecting the number of combinations representing the scale of error, scarcely makes a perceptible difference in the result, after the exclusion of the constant error: and that we may safely represent the probable error of the mean result of n observations, by the expression $.85\frac{e}{\sqrt{n}}$, e being the mean of all the actual errors.

We might obtain a conclusion nearly similar by considering the sum of the squares of the errors, amounting always to $n\,\varepsilon^2$: but besides the greater labour of computing the sum of the squares of the errors of any series of observations, the method, strictly speaking, is somewhat less accurate, since the amount of this sum is affected in a slight degree by any error which may remain in the mean, while the simple sum of the errors is wholly exempted from this uncertainty. In other respects the results here obtained do not materially differ from those of Legendre, Bessel, Gauss, and Laplace: but the mode of investigation appears to be more simple and intelligible.

It may therefore be inferred from these calculations, first, that the original conditions of the probability of different errors, though they materially affect the observations themselves, do not very greatly modify the nature of the conclusions respecting the accuracy of the mean result, because their effect is comprehended in the magnitude of the mean error from which those conclusions are deduced: and secondly, that the error of the mean, on account of this limitation, is never likely to be greater than six sevenths of the mean of all the errors, divided by the square root of the number of observations. But though it is perfectly true, that the probable error of the mean is always somewhat less than the mean error divided by the square root of the number of observations, provided that no constant causes of error have existed; it is still very seldom safe to rely on the total absence of such causes; especially as our means of detecting them must be limited by the accuracy of our observations, not assisted, in all instances, by the tendency to equal errors on either side of the truth: and when we are comparing a series

of observations made with any one instrument, or even by any one observer, we can place so little reliance on the absence of some constant cause of error, much greater than the probable result of the accidental causes, that it would in general be deceiving ourselves even to enter into the calculation upon the principles here explained: and it is much to be apprehended, that for want of considering this necessary condition, the results of many elegant and refined investigations, relating to the probabilities of error, may in the end be found perfectly nugatory.

These are cases in which some little assistance may be derived from the doctrine of chances with respect to matters of literature and history: but even here it would be extremely easy to pervert this application in such a manner, as to make it subservient to the purpose of clothing fallacious reasoning in the garb of demonstrative evidence. Thus if we were investigating the relations of two languages to each other, with a view of determining how far they indicated a common origin from an older language, or an occasional intercourse between the two nations speaking them, it would be important to inquire, upon the supposition that the possible varieties of monosyllabic or very simple words must be limited by the extent of the alphabet to a certain number; and that these names were to be given promiscuously to the same number of things, what would be the chance that 1, 2, 3 or more of the names would be applied to the same things in two independent instances.

Now we shall find, upon consideration, that for n names and n things, the whole number of combinations, or rather permutations of the whole nomenclature would be $m = 1 . 2 .$

$8 \ldots n$; and that of these the number in which no one name agreed would be $a_n = m - a_1 - n \cdot \frac{n-1}{2} \cdot a_2 - n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot a_3 \ldots - n \cdot a_{n-1}$; each term expressing the number of agreements in $n, n-1, n-2 \ldots$ instances only, and being made up of all the combinations of so many out of n things, each occurring as many times as all the remaining ones can disagree. Hence we may easily obtain the successive values of a from each other, the first being obviously 1, as a single name can only be given in one way to a single thing, therefore,

$a_1 = 1$

$a_2 = 2 - 1 = 1$

$a_3 = 6 - 1 - 3 = 2$

$a_4 = 24 - 1 - 6 - 8 = 9$

$a_5 = 120 - 1 - 10 - 20 - 45 = 44$

$a_6 = 720 - 1 - 15 - 40 - 135 - 264 = 265$

$a_7 = 5040 - 1 - 21 - 70 - 315 - 924 - 1855 = 1854$

$a_8 = 40320 - 1 - 28 - 112 - 630 - 2464 - 7420 - 14832 = 14833$

$a_9 = 362880 - 1 - 36 - 168 - 1134 - 5544 - 22260 - 66744 - 133497 = 133496$

$a_{10} = 3628800 - 1 - 45 - 240 - 1890 - 11088 - 55650 - 222480 - 667485 - 1334960 = 1334961$

From this computation it may be inferred, that, for 10 names, the probabilities will stand thus:

No coincidence	.367880	One or more	.632120
One only	.367880	Two or more	.264240
Two only	.183941	Three or more	.080300
Three only	.061309	Four or more	.018991
Four only	.015336	Five or more	.003655
Five only	.003056	Six or more	.000599
Six only	.000521	Seven or more	.000078

Seven only	.000066	Eight or more	.000013
Eight only	.000012	Nine or Ten	.0000003

The same results may be still more readily obtained from the supposition that n is a very large number; for then, the probability of a want of coincidence for a single case being $\frac{n-1}{n}$, the probability for two trials will be $\left(\frac{n-1}{n}\right)^2$, and for the whole n, $\left(\frac{n-1}{n}\right)^n = \left(1-\frac{1}{n}\right)^n$: but the hyperbolical logarithm of $1-\frac{1}{n}$ being ultimately $-\frac{1}{n}$, that of $\left(1-\frac{1}{n}\right)^n$ will be -1, consequently the probability of no coincidence will be $\frac{1}{2.718282} = .3678794$: and if n is increased by 1, each of these cases of no coincidence will afford 1 of a single coincidence: if by two, each will afford one of a double coincidence, but half of them will be duplicates; and if by three, the same number must be divided by 6, since all the combinations of three would be found six times repeated. We have therefore for

No coincidence	.3678794	One or more	.6321206 $= \frac{2}{3} -$
One only	.3678794	Two or more	.2642412 $= \frac{1}{4} +$
Two only	.1839397	Three or more	.0803015 $= \frac{1}{12} -$
Three only	.0613132	Four or more	.0189883 $= \frac{1}{53}$
Four only	.0153283	Five or more	.0036600 $= \frac{1}{273}$
Five only	.0030657	Six or more	.0005943 $= \frac{1}{1683}$
Six only	.0005109	Seven or more	.0000834 $= \frac{1}{12000}$
Seven only	.0000730	Eight or more	.0000105 $= \frac{1}{95200}$

It appears therefore that nothing whatever could be inferred with respect to the relation of two languages from the coincidence of the sense of any single word in both of them; and that the odds would only be 8 to 1 against the agreement of two words: but if three words appeared to be identical, it

would be more than 10 to 1 that they must be derived in both cases from some parent language, or introduced in some other manner; six words would give near 1700 chances to 1, and 8 near 100,000: so that in these last cases the evidence would be little short of absolute certainty.

In the Biscayan, for example, or the ancient language of Spain, we find in the vocabulary accompanying the elegant essay of Baron W. VON HUMBOLDT, the words *berria*, new; *ora*, a dog; *guchi*, little; *oguia*, bread; *otsoa*, a wolf, whence the Spanish *onza*; and *zazpi*, or, as LACROZE writes it, *shashpi*, seven. Now in the ancient Egyptian, new is BERI; a dog, UHOR; little, KUDCHI; bread, OIK; a wolf, UONSH; and seven, SHASHF; and if we consider these words as sufficiently identical to admit of our calculating upon them, the chances will be more than a thousand to one, that, at some very remote period, an Egyptian colony established itself in Spain; for none of the languages of the neighbouring nations retain any traces of having been the medium through which these words have been conveyed.

On the other hand, if we adopted the opinions of a late learned antiquary, the probability would be still incomparably greater that Ireland was originally peopled from the same mother country: since he has collected more than 100 words which are certainly Egyptian, and which he considers as bearing the same sense in Irish; but the relation, which he has magnified into identity, appears in general to be that of a very faint resemblance: and this is precisely an instance of a case, in which it would be deceiving ourselves to attempt to reduce the matter to a calculation.

The mention of a single number, which is found to be in-

disputably correct, may sometimes afford a very strong evidence of the accuracy and veracity of a historian. If the number were indefinitely large, the probability that it could not have been suggested by accident would amount to an absolute certainty: but where it must naturally have been confined within certain moderate limits, the confirmation, though somewhat less absolute, may still be very strong. For example, if the subject were the number of persons collected together for transacting business, it would be a fair presumption that it must be between 2 or 3 and 100, and the chances must be about 100 to 1 that a person reporting it truly must have some good information; especially if it were not an integral number of tens or dozens, which may be considered as a species of units. Now it happens that there is a manuscript of DIODORUS SICULUS, which, in describing the funerals of the Egyptians, gives 42 for the number of persons who had to sit in judgment on the merits of the deceased: and in a multitude of ancient rolls of papyrus, lately found in Egypt, it may be observed, that 42 personages are delineated, and enumerated, as the judges assisting Osiris in a similar ceremony. It is therefore perfectly fair to conclude from this undeniable coincidence, that we might venture to bet 100 to 1, that the manuscript in question is in general more accurate than the others which have been collated; that DIODORUS SICULUS was a well informed and faithful historian; that the graphical representations and inscriptions in question do relate to some kind of judgment; and lastly, that the hieroglyphical numbers, found in the rolls of papyrus, have been truly interpreted.

2. *On the mean density of the earth.*

It has been observed by some philosophers, that the excess of the density of the central parts of the earth, above that of the superficial parts, is so great as to render it probable that the whole was once in a state of fluidity, since this is the only condition that would enable the heaviest substances to sink towards the centre. But before we admit this inference, we ought to inquire, how great would be the effect of pressure only in augmenting the mean density, as far as we can judge of the compressibility of the substances, which are the most likely to be abundant, throughout the internal parts of the structure.

Supposing the density at the distance x from the centre to be expressed by y, the fluxion dy will be jointly proportional to the thickness of the elementary stratum, or to its fluxion $-dx$, to the actual density y, and to the attraction of the interior parts of the sphere, which varies as $\frac{\int yx\,dx}{x^2}$; since the increment of pressure, and consequently that of density, depends on the combination of these three magnitudes: we have therefore $-ndy = ydx\frac{\int yx\,dx}{x^2}$; an equation which will readily afford us the value of y in a series of the form $1 + ax^2 + bx^4 + \ldots$.

In order to determine the coefficients, we must first find $\frac{\int yx\,dx}{x^2} = \frac{1}{3}x + \frac{1}{5}ax^3 + \frac{1}{7}bx^5 + \ldots$, and multiplying this by $(1 + ax^2 + bx^4 + \ldots)\,dx$, we obtain

$$-ny = -n - nax^2 - nbx^4 - ncx^6 - \ldots$$
$$= C + \tfrac{1}{2.3}x^2 + \left.\begin{matrix}\tfrac{1}{4.5}a\\ +\tfrac{1}{3.4}a\end{matrix}\right\}x^4 + \left.\begin{matrix}\tfrac{1}{6.7}b\\ +\tfrac{1}{5.6}a^2\\ +\tfrac{1}{3.6}b\end{matrix}\right\}x^6 + \ldots$$

Hence, by comparing the corresponding terms, we obtain

$C = -n$;		
$a = -.1666667n - 1$	Logarithm,	9.2218487
$b = .2222222n - 2$		8.3467875
$c = -.0296960n - 3$		7.4296867
$d = .0030815 4n - 4$		6.4887650
$e = -.0003407 43n - 5$		5.5324269
$f = .0000367495n - 6$		4.5652514
$g = -.0000038908 6n - 7$		3.5911459
$[h = .0000004062n - 8$		2.6087]
$[i = -.0000000420n - 9$		1.6232]
$[k = -.0000000043n - 10$		0.6335]

After the exact determination of the first seven coefficients, the next three are obtained with sufficient accuracy by means of the successive differences of the logarithms, compared with those of the natural numbers.

It happens very conveniently, that the conditions of the problem are such, as to afford a remarkable facility in deriving from this series another, which is much more convergent, and which gives us the hyperbolic logarithm of y; for since $-n\frac{dy}{y} = dx\frac{fyxrdx}{xx}$, and $\frac{fyxrdx}{xx} = \frac{1}{3}x + \frac{1}{5}ax^3 + \frac{1}{7}bx^5 + \ldots$, if we multiply this by dx, and take the fluent, we shall have $HL\,y = -\frac{1}{n}\left(\frac{1}{2.3}x^3 + \frac{1}{4.5}ax^4 + \frac{1}{6.7}bx^6 + \ldots\right)$.

We may determine the degree of compressibility corresponding to a given value of n, by comparing the equation $-n\frac{dy}{y} = dx\frac{fyxrdx}{xx}$, or $= dx.p$, with the properties of the modulus of elasticity M, which is the height of such a column of the given substance, that the increment of density y', occasioned by the additional weight of the increment x', is always

to y, as x' to M, or $\frac{y'}{y} = \frac{x'}{M}$, whence $-\frac{dy}{y} = \frac{dx}{M}$; consequently in the present case we have $-\frac{dx\rho}{n} = \frac{dx}{M}$; and $M = \frac{n}{\rho}$: and if we make $x = 1$ in the value of p, we shall obtain M in terms of the radius of the earth, considered as unity. When y is invariable, and n infinite, the density being uniform, ρ becomes $\frac{1}{3}$, and the mean density will always be expressed by $3p$, since the attractive force is simply as the mean density: and if we divide $3p$ by y, we shall have the relation of the mean density to the superficial density. The results of this calculation, for different values of n, are arranged in the table, which will be found sufficiently accurate for the purposes of the investigation, though not always correct to the last place of figures.

n	p	$M = \frac{n}{\rho}$	$3p$, mean density		y	$\frac{3p}{y}$, comp. den.
∞	.33333	∞	1.0000 =	1:1.0000	1.000	1.000
1	.30290	3.301	.9087	1.1005	.855	1.065
$\frac{1}{2}$	.27785	1.803	.8320	1.2019	.738	1.127
$\frac{1}{3}$	.25535	1.305	.7660	1.3054	.646	1.185
$\frac{1}{4}$	.23688	1.055	.7106	1.4071	.575	1.24
$\frac{1}{5}$	.22058	.907	.6617	1.5111	.510	1.30
$\frac{1}{6}$	.20616	.808	.6185	1.6168	.458	1.35
$\frac{1}{7}$	.194	.736	.582	1.72	.419	1.40
$\frac{1}{8}$	.183	.681	.549	1.82	.377	1.45
$\frac{1}{9}$	.172	.646	.516	1.94	.346	1.49
[$\frac{1}{10}$	.162	.617	.486	2.05	.320	1.52]
[$\frac{1}{11}$	.153	.594	.459	2.16	.298	1.55]
[$\frac{1}{12}$	.145	.575	.435	2.28	.28	1.57]
[$\frac{1}{20}$	.1	.5	.3	3.3	.17	1.8]

The reciprocals of the mean density are inserted, on account

of the simplicity of the progression which they exhibit, being in the first instance precisely equal to $1 + \frac{1}{108}$, and varying but slowly from this value.

Now if we suppose, with Mr. Laplace, the mean density of the earth to be to that of the superficial parts as 1.55 to 1, it appears from this table, that the height of the modulus of elasticity must be about .594; that is, more than 12 million feet, while the modulus of the hardest and most elastic substances, that have been examined, amounts only to about 10 million. It follows therefore, that the general law, of a compression proportionate to the pressure, is amply sufficient to explain the greater density of the internal parts of the earth; and the fact demonstrates, that this law, which is true for small pressures in all substances, and with regard to elastic fluids, in all circumstances, requires some little modification for solids and liquids, the resistance increasing somewhat faster than the density: for no mineral substance is sufficiently light and incompressible to afford a sphere of the magnitude of the earth, and of so small a specific gravity, without some such deviation from the general law. A sphere of water would be incomparably more dense, and one of air would exceed this in a still greater proportion: indeed, even the moon, if she is really perforated, as has sometimes been believed, and contains cavities of any considerable depth, would soon have absorbed into her substance the whole of her atmosphere, supposing that she ever had one. It may be objected, that the resistance of solids to actual compression may possibly be considerably greater than appears in our experiments, since we are not absolutely certain that they

do not extend in a transverse direction, when we compress them in a longitudinal one, as is obviously the case with some soft elastic substances: but this objection is removed by the experiment on the sound of ice, which affords, either accurately or very nearly, the same resistance to compression as a portion of water confined in a strong vessel; and this it could not do, if the particles of ice were allowed to expand laterally under the operation of a compressing force.

Mr. LAPLACE's conclusion, respecting the precise proportion of the densities, is indeed derived from another supposition respecting their variation, and would be somewhat modified by the adoption of this theory; it would not, however, be so materially altered, as by any means to invalidate the general inference. It would therefore be proper to revise the calculations derived from the lunar motions and the ellipticity of the earth, and to employ in them a variation of density somewhat resembling that which is here investigated. Indeed without reference to the effects of compressibility, it is obviously probable that the density of the earth should vary more considerably in a given depth towards the surface than near the centre, although the calculation, upon Mr. LAPLACE's more simple hypothesis, of a uniform variation, is much less intricate. It would however be justifiable, as a first approximation, to reject those terms of the series which would vanish if n and x were very small, and to make $y = 1 + ax^n$; and indeed this formula has in one respect an advantage over the series, as it seems to approach more nearly to the law of nature, in expressing a resistance somewhat greater towards the centre, where the density is most augmented: we have then, if the superficial density be to the

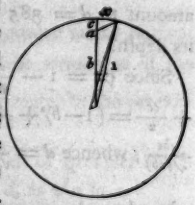
mean as 1 to q, $q = \frac{1+\frac{1}{2}a}{1+a}$, whence $a = -\frac{q-1}{q-.6}$; and if $q = 1.55$, $a = -.58$, affording an expression which is, in all probability, accurate enough for every astronomical purpose.

If the variation of density were supposed to proceed equably with the variation of quantity, it would obviously be as the square of the distance from the centre, and the density would be as $1 - ax^2$, the mean density being found at the surface of a sphere containing half as much as the whole earth; and this might be considered as the most natural hypothesis, if we disregarded the effects of compression: but the arithmetical progression of densities, from the centre to the surface, seems in every way improbable.

3. *On the irregularities of the earth's surface.*

A. If we suppose the plumb line to deviate from its general direction on account of the attraction of a circumscribed mass, situated at a moderate depth below the earth's surface, the distance of the two points of greatest deviation from each other will be to the depth of the attracting point as 2 to $\sqrt{2}$.

Let the magnitude of the additional mass be to that of the earth as a to 1, and let its distance from the centre be b; then supposing the earth a sphere, and its radius unity, and calling the angular distance of any point from the semidiameter passing through the mass x, the linear distance from the mass will be $\sqrt{(f^2x + (\varsigma x - b)^2)} = \sqrt{(f^2x + \varsigma^2 x - 2b\varsigma x + b^2)} = \sqrt{(1 + b^2 - 2b\varsigma x)}$; consequently the disturbing attraction will be $\frac{a}{1+bb-2b\varsigma x}$: but the sine of the angle subtended by the two



centres of attraction will be to their distance b as fx to the oblique distance $\surd(1+b^2-2b\zeta x)$; it will therefore be expressed by $\frac{bfx}{\surd(1+bb-2b\zeta x)}$; and the sine of the very small angular deviation of the joint force from the radius will be to the line measuring the disturbing force as this last sine to the radius, the difference of the third side of the triangle from the radius being inconsiderable; consequently the deviation will be every where expressed by $\frac{abfx}{(1+bb-2b\zeta x)^{\frac{3}{2}}}=d$. Now in order to find where this is greatest, we must make its fluxion vanish, and $o=\frac{\zeta x dx}{(1+bb-2b\zeta x)^{\frac{3}{2}}}-\frac{3}{2}\cdot\frac{fx2bfxdx}{(1+bb-2b\zeta x)^{\frac{5}{2}}}$, $\zeta x(1+b^2-2b\zeta x)=3bf^2x$, $3b\zeta^2x-2b\zeta^2x+(1+b^2)\zeta x=3b$, $\zeta^2x+\frac{1+bb}{b}\zeta x=3$, and $\zeta x=\surd\left(3+\left[\frac{1+bb}{2b}\right]^2\right)-\frac{1+bb}{2b}$; but, making $b=1-c$, $\frac{1+bb}{2b}$ becomes $\frac{1+1-2c+ee}{2b}=1+\frac{ee}{2b}$; and c being very small, ζx will be $\surd\left(4+\frac{ee}{b}\right)-1-\frac{ee}{2b}=2+\frac{ee}{4b}-1-\frac{ee}{2b}=1-\frac{ee}{4b}$; whence $fx=\surd\left(1-\left[1-\frac{ee}{4b}\right]^2\right)=\surd\left(1-1+\frac{ee}{2b}\right)=\frac{e}{\surd(2b)}$. or simply $\surd\frac{1}{2}e$, and $e=\surd 2fx$.

B. The sine of the greatest deviation of the plumb line will amount to $d=.385\frac{a}{cc}$, a being the disturbing mass, and c its depth.

Since $\zeta x=1-\frac{ee}{4b}$, $2b\zeta x=2b-\frac{ee}{2}$, and $1+bb-2b+\frac{ee}{2}=(1-b)^2+\frac{ee}{2}=c^2+\frac{-ee}{2}=\frac{1}{2}c^2$; and $abfx$ becomes $\frac{abc}{\surd(2b)}$, whence $d=\frac{abc}{\surd\left(2b\cdot\frac{27}{8}\right)c^3}=\frac{2a\surd b}{\surd 27\,cc}=.385\frac{a\surd b}{cc}$, or simply $.385\frac{a}{cc}$; also $a=2.618c^2d$, and $c=\surd\left(.385\frac{a}{d}\right)$. If the density were doubled throughout the extent of a sphere touching the surface internally, the radius being c, we should

have $a = c^3$ and $d = .385c$, and $c = 2.6d$: but this is a much greater increase of density than is likely to exist on a large scale: so that c must probably in all cases be considerably greater than this.

C. The greatest elevation of the general surface above the sphere will be $\frac{a}{c}$, on the supposition that the mutual attraction of the elevated parts may safely be neglected.

The fluxion of the elevation is as the fluxion of the arc and as the deviation d conjointly; it will therefore be expressed by $\frac{ab\dot{x}dx}{(1+bb-2b\varsigma x)^{\frac{1}{2}}}$. Now the fluxion of $\frac{1}{\sqrt{(1+bb-2b\varsigma x)}}$ is $-\frac{1}{2}\frac{2b\dot{x}dx}{(1+bb-2b\varsigma x)^{\frac{3}{2}}}$, consequently the fluent of the elevation will be $\frac{-a}{\sqrt{(1+bb-2b\varsigma x)}}$: and while ςx varies from 1 to -1, this fluent will vary from $\frac{-a}{1-b}$ to $\frac{-a}{1+b}$, the difference being $a\left(\frac{1}{1-b}-\frac{1}{1+b}\right) = a\left(\frac{1}{c}-\frac{1}{2-c}\right) = a\left(\frac{2-c-c}{2c-cc}\right)$, or simply $\frac{a}{c}$, since c is an extremely small fraction. This quantity comprehends indeed the depression on the remoter side of the sphere, which would be required to supply matter for the elevation; but it is obvious that such a depression must be wholly inconsiderable.

D. The diminution of gravity to the centre at the highest point is $\frac{2a}{c}$, while the increase from the attraction of the disturbing mass is nearly $\frac{a}{4c}$, which is greater in the proportion that half the radius bears to c.

E. The increase of gravity, at the point of greatest deviation, is to the deviation itself, or its sine d, as $\sqrt{2}$ to 1.

For the deviation is the measure of the horizontal attraction of the disturbing mass, which is to its vertical attraction

as $\int x$ to c, or as $\sqrt{\frac{1}{2}}$ to 1. Thus if d were 5″, or $\frac{5}{206265}$, the horizontal force would be $\frac{7.071}{206265} = \frac{1}{29170}$, and the acceleration of a pendulum $\frac{1}{58340}$ or 1.5″ of time in a day. It is true that a part of the deviation might depend on a defect of density as well as on an excess; but this defect could not amount to any great proportion of the whole, while the excess above the general density might easily be much more considerable, so that the acceleration of the pendulum could scarcely be *less* than a second in a day, if the greatest deviation of the plumb line were 5″; and if the deviation were 5″ at any other place, there would be a *greater* acceleration than a second at a point more or less remote from it.

F. If there were an excess of density on one side, and a deficiency on the other, so as to constitute virtually two centres of attraction and repulsion, and supposing their distances to be equal, and such as to produce the greatest deviation, if the excess of density were twice as great as the deficiency, a deviation of 5″ would correspond to an acceleration of half a second; if 3 times as great, to $\frac{1}{4}$; if 4 times, to $\frac{2}{10}$; and if five, to a second.

It may perhaps be considered as an omission in this calculation, that the attraction of the parts of the earth's surface, elevated by means of the irregular gravitation, has not been included in it. But it depends on the supposition that we may adopt respecting the cause and date of the irregularity, whether or no we ought to consider it as likely to have occasioned such a general elevation; and it does not appear that the result of the computation would very materially alter our conclusions, though it would be somewhat laborious to go

through all its steps with precision. It would indeed be so much the more superfluous to insist on this minute accuracy, as variations so much more considerable in the form of the earth's surface are commonly neglected: for example, in the allowance made for the reduction of different heights to the level of the sea, which has usually been done without any consideration of the attraction of the elevated parts, interposed between the general surface and the place of observation. It is however obvious, that if we were raised on a sphere of earth a mile in diameter, its attraction would be about $\frac{1}{6000}$ of that of the whole globe, and instead of a reduction of $\frac{1}{2000}$ in the force of gravity, we should obtain only $\frac{1}{6000}$, or three fourths as much: nor is it at all probable that the attraction of any hill a mile in height would be so little as this, even supposing its density to be only two thirds of the mean density, of the earth: that of a hemispherical hill would be more than half as much more, or in the proportion of 1.586 to 1; and it may easily be shown, that the attraction of a large tract of table land considered as an extensive flat surface, a mile in thickness, would be three times as great as that of a sphere a mile in diameter: or about twice as great as that of such a sphere of the mean density of the earth: so that, for a place so situated, the allowance for elevation would be reduced to one half: and in almost any country that could be chosen for the experiment, it must remain less than three fourths of the whole correction, deduced immediately from the duplicate proportion of the distances from the earth's centre. Supposing the mean density of the earth 5.5, and that of the surface 2.5 only, the correction, for a tract of table land, will be reduced to $1-\frac{2.5}{5.5}=\frac{29}{44}$, or $\frac{66}{100}$ of the whole.

4. EULER's *formula for the rolling pendulum.*

I beg leave to observe, in conclusion, with regard to Mr. LAPLACE's theorem for the length of the convertible-pendulum rolling on equal cylinders, that its perfect accuracy may readily be inferred, without any limitation of the form of the pendulum, or of the magnitude of the cylinders, from the general and elegant investigation of EULER, which also affords us the proper correction for the arc of vibration. This admirable mathematician has demonstrated, in the sixth volume of the Nova Acta Petropolitana, for 1788, p. 145, that if we put k for the radius of gyration with respect to the centre of gravity, a for the distance of the centre of gravity from the centre of the cylinder, c for the radius of the cylinder, h^2 for $k^2+(a-c)^2$, and b for the sine of half of any very small arc of semivibration, we shall have, for the time of a complete oscillation, $\frac{\pi b}{\sqrt{(2ag)}}+\frac{\pi bb(bb+4ac)}{4b\sqrt{(2ag)}}$, and ultimately, if $b=0$, $\frac{\pi b}{\sqrt{(2ag)}}$ only, which, for a simple pendulum, of the length a, k and c both vanishing, becomes $\frac{\pi\sqrt{a}}{\sqrt{(2g)}}$, and for any other length l, $\frac{\pi\sqrt{l}}{\sqrt{(2g)}}$; consequently, making $\frac{\pi\sqrt{l}}{\sqrt{(2g)}}=\frac{\pi b}{\sqrt{(2ag)}}$, we have $\sqrt{l}=\frac{b}{\sqrt{a}}$, and $al=hh=k^2+a^2-2ac+c^2$. Now if we find another value of a, which will fulfil the conditions of the equation, all the other quantities concerned remaining unaltered, and add the two values together, we shall have the distance of the centres of the two cylinders corresponding to the length l of the equivalent pendulum; but since $a^2-(l+2c)\,a=-k^2-c^2$, we have $a-\frac{1}{2}l-c=\pm\sqrt{\ldots}$, and $a=\frac{1}{2}l+c\pm\sqrt{\ldots}$, so that the sum of the two values of

a must be $l + 2c$, that is, the distance of the centres of the cylinders must exceed the length l by twice the radius, and l must be precisely equal to the distance of their surfaces.

Believe me, dear Sir,
very sincerely your's,

THOMAS YOUNG.

Welbeck Street, 29 Dec. 1818.

VI. *On the anomaly in the variation of the magnetic needle as observed on ship-board. By* William Scoresby, *jun. Esq. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. G. C. B. P. R. S.*

Read February 4, 1819.

THE anomalies discovered in magnetical observations conducted on ship-board, were usually attributed to the imperfection of the azimuth compass, until Captain FLINDERS, in his modest and enlightened paper on this subject, published in the Philosophical Transactions, suggested that they were probably owing to the concentration of the magnetic influence of the iron, made use of in the construction of the ship. The truth of this suggestion, and the accuracy of his observations, have since met with full confirmation, and his practical rules founded thereon have received additional support, from the "Essay" of Mr. BAIN "on the variation of the Compass," published last year.

As I have been materially anticipated by Mr. BAIN, in a series of observations on the variation of the compass,* which

* The azimuths contained in the following table were taken, either by the needle of a theodolite, or by a compass fitted up at sea, for the purpose, with a card made extremely light, and a bar fastened edgewise to it, by two brass screws, *a a*, as in the annexed sketch. The compass being small, the card light, and the needle very powerful, owing to the thickness of its ends, it performed considerably better than an expensive azimuth compass of larger dimensions, which indeed was so sluggish and erroneous in its indications, that I could make no good use of it.

I conducted on the coast of Spitzbergen, in the years 1815 and 1817, it will be unnecessary here to enter into the detail of these observations or enlarge upon the probable cause of the anomalies observed; it may be sufficient to give a table of the most accurate of my observations, and annex to it the few general inferences which were drawn from it, during the voyage in which most of the observations were made, together with such remarks on each inference as seemed to me calculated for its elucidation. I shall, however, just premise, that I am not unconscious of the great liability to error in observations of this kind, and of the variety of causes (arising out of the unequal distribution of iron in different ships, whereby numerous local attractions are formed) which contribute to the multiplication of those errors: it is, therefore, with the greatest deference that I submit these deductions, particularly as I conceive it will require observations to be made under a vast variety of circumstances, and in many different vessels, before *correct* and satisfactory conclusions can be drawn. It is *only* then as a step towards facilitating such general conclusions, the importance of which to our maritime concerns is so obvious, that I presume to offer these observations and remarks.

Table of Magnetical Observations, made on board of the ship Esk, in voyages to the Greenland or Spitzbergen Whale Fishery; with a view to investigate the laws by which the anomalies, or errors, discovered in the indications of the Mariner's Compass on ship-board, are regulated.

No.	Date.	Latitude.	Longitude.	Time. [Ch. Chronometer.] [Ap. Apparent.]	Sun's Azimuth. Observed.	Sun's Azimuth. True.	Variation. Apparent or observed.	Variation. Supposed true.	Errors produced in the observed variation by the attraction of the ship on compass needle. Errors attributed to the position of the ship's head. [Diff. of col. VIII. & IX.]	Differences produced by a change in position of the ship's head. [From col. VIII.]	Differences produced by a change in the position of the compass. [From col. VIII.]	Ship's head by binnacle compass.	Situation of the compass by which the Sun's azimuth was observed. [Col. VI.]
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	XIII.	
	1815.												
1	June 30	77° 19′ N.	12° 15′ E.	*Ap.* Noon.	S. 7° W.	South.	7° W.	21° W.	14° 0′	- - -	31° 0′	S. by E.	Starboard side of the main deck.
2	- -	- -	- -	- -	38	South.	38	21	17 0			S. by E.	Larboard ditto.
3	- -	- -	- -	- -	21	South.	21	21	0 0	- -	17 0	S. by E.	Centre of main hatches.
	1817.												
4	June 6	77 56	0 15 W.	*Ch.* 6h 54′ P.M.	N. 36 W.	N. 73° W.	37	32.30	4 30	8° 15′	- -	W. ½ S.	On the binnacle amidships of the
5	- -	- -	- -	7 28	35½	64 30	28.45	32 30	3 45			East.	quarter-deck, 10 ft. from taffrail.
6	- -	- -	- -	7 28	45	64 30	19.30	32.30	13 0	- -	9 15	East.	Compass on a stand on the middle
7	- -	- -	- -	6 54	27½	73 0	45.30	32.30	13 0	26 0	- -	W. ½ S.	of the quarter-deck, 6 feet abaft
8	- -	- -	- -	7 32	33	63 30	30.30	32.30	2 0	- -	- -	E. ½ S.	capstern spindle. Starboard side of the main-deck.
9	- -	- -	- -	7 33	31	63 15	32.15	32.30	0 15	- -	1 45	E. ½ S.	Larboard side of the main-deck.
10	June 19	77 36	2 35 W.	*Ap.* 11 38	S. 25 W.	S. 5 30 E.	30.30	36. 0	5 30	- -	- -	E. by S.	On the binnacle, 10 ft. from taffrail.
11	- -	- -	- -	11 20	27	10 0	37. 0	36. 0	1 0	6 30	- -	N.	Ditto.
12	- -	- -	- -	12 3	34½	S. 0 45 W.	34. 0	36. 0	2 0	3 0	- -	NE. ½ E.	Ditto.
13	—— 28	76 24	4 45 W.	*Ch.* 7 34	N. 46 W.	N. 68 11 W.	22.21	37. 0	14 49	- -	- -	NNE.	Starboard side of the main-deck.
14	- -	- -	- -	7 38	16½	67 11	50.41	37. 0	13 41	- -	28 30	NNE.	Larboard ditto.
15	July 16	76 6	0 30 W.	*Ch.* 6 33	60	79 54	19.54	33. 0	13 6	- -	- -	South.	Starboard side of the main-deck.
16	- -	- -	- -	6 36	33	79 9	46. 9	33. 0	13 9	- -	26 15	South.	Larboard ditto.
17	- -	- -	- -	6 31	46	80 24	34.24	33. 0	1 24	- -	11 45	South.	Centre of main-hatches.
18	- -	- -	- -	6 44	53	77 9	24. 9	33. 0	8 51	- -	- -	South.	Centre of fore-hatches.
19	- -	- -	- -	6 43	41	77 24	36.24	33. 0	3 24	- -	12 15	South.	Larboard side fore-castle near the windlass end.
20	- -	- -	- -	6 41	70	77 54	7.54	33. 0	25 6	- -	28 30	South	Starboard ditto.

From these observations, and from the assistance afforded by the lucid remarks of Captain FLINDERS, the inferences which follow are deduced.

1. In the construction of every ship, a large quantity of iron being used, the portions thereof which have a perpendicular position, such as standard and hanging knees, the nails and bolts in the deck, the capstern spindle, flukes of the anchors (when at sea), chain-plates, iron stanchions and riders; the eye bolts, transom bolts, joint bolts, &c. of gun carriages, and possibly the upper surfaces of the guns themselves, &c. &c. have a tendency to become magnetical, the upper ends being *south* poles and the lower *north* poles, in this hemisphere, where the *north* end of the needle dips, but the contrary in the southern hemisphere, where the *south* end of the needle dips.

2. The combined influence of the iron distributed through all parts of the ship, seems to be concentred into a kind of magnetic *focus of attraction*, the principal south pole of which being upward in the northern hemisphere, is probably situated, in general, near the middle of the upper deck, but nearer to the stem than the stern.

Wrought iron having a much greater attraction for the magnetic needle than cast iron, the anchors, which usually lie about the bows, possess much more influence than guns; hence, the focus of attraction lies nearer to the bows than to the stern.

3. This focus of attraction so influences the compass needle, that it is subject to an *anomaly*, or variation from the true meridian, different from what is observed by a compass on shore; the north point of the compass being constantly drawn towards the focus of attraction, which appears to be a

south pole in north dip; and the south point being attracted in south dip, where the focus of attraction probably becomes a north pole.

The phenomenon of a ship appearing to lie nearer the wind when beating to the northward, with the wind at north, than when beating to the southward, with a southerly wind, was observed by my father at least 20 years ago, which phenomenon he attributed to the "attraction of the ship upon the compass;" and ever since the year 1805, I have been in the habit of allowing only 2 to 2¼ points variation on the passage outward to Greenland, with a northerly or north-easterly course, but generally 3 points variation on the homeward passage when the course steered was S. W. or S. W. b. W. Without this difference of allowance, a Greenland ship outward bound will be generally found to be to the eastward of the reckoning, and homeward bound will be even 4 or 5 degrees to the eastward of it.

4. This anomaly in the variation of the compass, occasioned by the attraction of the iron in the ship, is liable to change with every alteration in the dip of the needle, in the position of the compass, or in the direction of the ship's head.

If the intensity of the terrestrial magnetism be not equal in all parts of the globe, then the anomaly in the variation of the compass will be also liable to change with every alteration in the magnetic influence of the earth. This is a point of such importance, I conceive, in the science of magnetism, that I was very anxious to procure a dipping needle on my last voyage to Greenland, to ascertain whether the magnetism of the earth, by which the dipping needle is influenced, be not greater near the magnetic pole, than it is in England.

If it be equal, the oscillations of the same dipping needle would be performed, circumstances as to temperature and "local attraction" being the same, in equal spaces of time in both places; but if the magnetic power in either place be greater, the oscillations of the needle would there be quicker. The number of vibrations of a horizontal needle, performed in a certain space of time in Greenland, is to the number performed in an equal space of time in England as 5 to 6, each longer vibration in England being performed in 5 seconds, and in Greenland in .6. No alteration was observed in the time required for each vibration, whether the temperature was high or low, but I think in a low temperature the vibrations performed by the needle before it stopped were fewer.

5. The anomaly of variation bears a certain proportion to the dip of the needle, being greatest where the dip is greatest, diminishing as the dip decreases, and disappearing altogether on the magnetic equator.

Captain FLINDERS ascertained, that the medium error or anomaly for 8 points deviation of the Investigator's head, on either side of the magnetic meridian, was very nearly $\frac{1}{20}$ of the dip, .05 the decimal expression of which, he considered to be the common multiplier to the dip, for obtaining the radius of error at any situation in the southern hemisphere; and .058 to be the common multiplier, from England to the magnetic equator. This, however, can only be correct within certain limits, as on the magnetic pole, where the anomaly would probably be equal to the dip, or 90°, the decimal multiplier would require to be increased to 1.0. Hence it has been suggested, by an officer on board one of the vessels now in search of a north-west passage, that in those parts of the globe where the dip is 90°, the compass needle would pro-

bably always stand N. and S., by the attraction of the ship. This position clearly follows from the inference above, provided the compass be placed near the ship's stern in midships; but if placed as described in inference No. 8, the ship's head by the compass on the starboard side of the main deck, would always appear to be *east*, and on the larboard side *west*.

6. A compass placed near the stern, amidships of the quarter-deck, is subject to the greatest anomaly or deflection from the magnetical meridian, when the ship's course is about west or east; because the focus of attraction then operates at right angles to the position of the compass needle; but the anomaly disappears when the course is about north or south, because the focus of attraction is then in a line with, or parallel to, the compass needle, and consequently has no power to deflect it from its direct position. [See Observations, No. 4, 10, 11, and 12 of the prefixed table.]

This situation for the *binnacle* is deemed one of the best in the ship, and is very properly preferred. Being abaft the focus of attraction, the north point of the compass, in this magnetic hemisphere, is always attracted forward, and the errors at equal distances from the magnetic meridian, in the same dip, are alike in quantity both on easterly and westerly courses, and always towards the north; the correction, when applied to the apparent course, must therefore be towards the south, to give the true course steered. Thus in high northern latitudes, where the anomaly is great, (say 20°, or 10 degrees on each side of the magnetic meridian) a ship steering west by the compass 100 leagues, and then east 100 leagues, instead of coming to the place from whence she started, will be 10¼ miles to the southward of it.

7. The greatest anomaly with the compass in the position

last described, being ascertained by observation, the error on every other point of the compass may be easily calculated; the anomalies produced by the attraction of the iron in the ship, being found to be proportionate to the sines of the angles between the ship's head and the magnetic meridian.

Captain FLINDERS's rule is—As the sine of eight points (or radius) is to the sine of the angle between the ship's head and the magnetic meridian (or sine of the course reckoned from south or north) so is the anomaly found at east or west by observation, to the anomaly on the course steered; or, the anomaly on any other course being found by observation, the error on that position of the ship's head "would be to the error at east or west, at the same dip, as the sine of the angle between the ship's head and the magnetic meridian, to the sine of eight points, or radius."

8. A compass placed on either side of the ship's deck, directly opposite to, or abreast of, the focus of attraction, gives a correct indication on an east or west course, but is subject to the greatest anomaly when the ship's head is north or south; and being here nearer the focus of attraction, the anomaly is much greater than that observed on an east or west course with the compass placed in the binnacle near the ship's stern.

This inference is founded on observations, No. 1, 2, 3, 8, 9, 13, 14, 15, 16, and 17, of the prefixed table. The latter part of the inference, namely, that the greatest anomaly occurs here when the ship's head is north or south, is fully and uniformly established; but the former part rests only on the authority of observations No. 8 and 9, though it derived

additional support from several observations which I have excluded, because neither the sun, nor any other distant object, calculated for proving the accuracy of the observations and determining the clear effect of the "local attraction," was visible.

9. A compass placed within six or eight feet of a capstern spindle, or anchor, or other large mass of wrought iron, foregoes, in a great measure, the influence of the focus of attraction, and submits to that of the nearer body of iron.

The effect of this is various, according to the relative position of the compass and the iron. When the compass is placed directly *abaft* the body of iron, the influence is similar to, but greater than, that of the focus of attraction on a compass placed near the stern, as described in inference No. 6. [See Table of observations prefixed, No. 6 and 7.] When placed directly *before* it, the anomaly is similar in quantity, but has its sign reversed; and when placed on either side of the mass of iron, the influence corresponds more nearly with that of the focus of attraction on a compass placed in the sides of the ship opposite to it, as described in inference No. 8. A compass placed upon the *drum head* of the capstern, any where out of the centre, will have its north point so forcibly attracted by the upper end or south pole of the spindle, that the ship's head may be made to appear to be directed to any point whatever, at the pleasure of the experimenter. I have sometimes excited the astonishment of my officers by taking the binnacle compass and so placing it on the capstern head, that the ship has appeared to be steering a course directly contrary to that intended.

10. When the iron in a ship is pretty equally distributed

throughout both sides, so that the focus of attraction occurs in midships, a compass placed on the midship line of the deck (drawn longitudinally) will be free from any anomaly from one end of the ship to the other, when the course is north or south; but on every other course an anomaly will generally appear, increasing as the angle between the ship's head and the magnetic meridian increases, until the error is at a maximum, when the course is east or west.

The unequal distribution of iron in the ship, on board of which I made all my experiments, prevented the above effect from being realized. A blacksmith's shop was situated between decks, on the larboard side of the fore hatchway. It was lined with sheet iron, and besides the armourer's forge, vice, &c. contained a large quantity of other iron. The effect of this, together with the anchors, windlass necks, and other iron, was very remarkable on a compass placed in different parts of the deck near the foremast. [See Observations, 18, 19, and 20 of the prefixed table.]

11. As a compass placed on the midship line of the deck is subject to no anomaly fore and aft, in certain ships, on a north or south course [Inference No. 10], and as a compass in either side of the ship, opposite to the focus of attraction, shows no anomaly on a west or east course [Inference No. 8], the intersection of the line joining the two situations in opposite sides of the ship with the midship line traced fore and aft, will probably point out a situation directly over the top of the focus of attraction, where no anomaly on any course whatever will appear.

The Esk, in which I made my magnetical observations, had, as above stated, an armourer's forge near the larboard bow,

which with the varying position of large quantities of iron work, composing our whale fishing apparatus, contributed to vary this point where no anomaly is supposed to exist, and prevented me from ascertaining satisfactorily, at any time, its precise situation. I made indeed but very few observations with this view, and these I find neither establish nor refute the inference.

12. The anomaly of variation is probably the greatest in men of war, and in ships which contain large quantities of iron; but it exists in a very considerable degree also in merchantmen, where iron forms no part of the cargo, especially in high latitudes, where the dip of the needle is great.

WILLIAM SCORESBY, Jun.

Whitby, 3d November, 1818.

VII. *On the genus Ocythoë; being an extract of a letter from* Thomas Say, *Esq. of Philadelphia, to* Wm. Elford Leach, *M. D. F. R. S.*

Read February 4, 1819.

I HAVE before me a specimen of *ocythoë* in an *argonauta*, forming part of the collection of the Acad. of Nat. Sciences. It was taken from the stomach of a dolphin, which was caught in soundings on our Atlantic coast, and is in the most perfect state of preservation, not having suffered the slightest decomposition from gastric action.

It is sufficiently distinct from your *O. Cranchii*, as well as from the animal of *nautilus sulcatus* of KLEIN; and if the figure given by SHAW of the animal of *argonauta argo* has any pretensions to accuracy, it is most probably an unknown species.

I here attempt a description of it, and also submit a few remarks on the genus.

Ocythoë punctata.

Body pale, punctured with purplish; abdomen conic-compressed, vertical, semifasciate near the summit, with a profoundly indented transverse line; arms much longer than the body, attenuated, filiform at their tips, alated; membranes rounded.

Inhabits the Atlantic ocean near the North American coast.

Descrip. Abdomen conical, slightly compressed, nearly vertical with respect to the disk of the head, with a profoundly indented transverse line, which extends half round, near the summit. *Arms* attenuated, much longer than the body, filiform towards the tip, slightly varied with brassy,

inferior ones, when extended double the length of the body; *suckers* alternate, becoming gradually smaller towards the extremities of the arms, where they are very minute; *membranes* of the anterior arms rounded or suborbicular, extending half way to the base of the arms; periphery occupied by the attenuated portion of the arm, which near its extremity passes upon the disk of the membrane, and terminates abruptly near the base of the expansion; the membrane is carinately decurrent on the inferior surface of the arm, near the base of which it terminates; the inferior surface of the membrane is brassy, and more numerously maculated than the superior surface, which is pale.

Length from the disk to the tip of the abdomen	2 inches.
——— of the abdomen - - -	$1\frac{1}{3}$
Greatest breadth ditto - - -	$1\frac{7}{10}$
Length of the alated arms - - -	$2\frac{3}{4}$
——— of those of the opposite side -	5

Eggs subovate, attached to a delicate pedicle by a small basilar tubercle. These fill the involuted spire in the specimen, besides a considerable portion of the body of the shell.

The suckers are very like those of the *O. Cranchii*, but the arms are much more elongated, and the abdomen longitudinal with respect to the head. This animal seems to be not unfrequently the prey of some of the larger fishes, for in addition to the instance above mentioned, Bosc informs us that in his passage between Europe and America, he found a specimen in the stomach of a *Coryphæna equiselis*, GMEL. but very much decomposed: and in the Museum of Mr. PEALE, in this city, a fine *argonauta* occurs, which was taken from the stomach of a shark.

With respect to the contested question relative to the parasitic nature of the animals of this genus, I believe the remark will hold good generally, if not absolutely, that those mulluscous animals that form the shell in which they reside, are more or less connected with it by muscular or membranaceous attachment, or by the permanent spiral form of the posterior part of the body; and that the body of the animal complies with the inequalities of the chamber of the shell, or rather that the shell is moulded upon the body, so as to be in contact with it in every part. So careful are they to fill the cavity to its very summit, that when from their increase of growth the apex of the shell is vacated in consequence of its straightness, either that part is removed by the animal, and additional calcareous matter is secreted to close the aperture thus formed, or it is permitted to remain, and the cavity is filled up by the same secretion; of the former process we have an instance in *Bulimus decollata*; and of the latter many instances occur, familiar to the knowledge of conchologists. The *Ocythoë* offers to our consideration a remote deviation from these ordinary laws which apply to the testaceous mollusca, inasmuch as it only resides in the last volution or body of the shell. In the specimen above described, the sides of the abdomen are slightly canaliculated, in conformity with the sculpture of the inner lateral surface of the shell; but it is worthy of remark, that the portion which corresponds with the most unequal part of the chamber, the carina, is not at all indented; which fact induces the supposition that the shell does not fit the body, and of course was not made for it, otherwise it does not seem probable that the body would

be remote from the shell in one part, and impressed by its asperities in another.

Such also is the form of the inferior part of the abdomen, that it never could have revolved in the cavity of the involuted spire; yet we have never been informed that the vacated spire has been either broken or solidified. Neither is there any attachment whatever between any part of the body and the including shell, by an organ appropriated to that office. In consequence of this organization, the ocythoë cannot adapt itself to the form of the cavity in which it rests, or secure itself there so completely as the well known parasitic paguri are enabled to do, in consequence of the pliability of their vesicular abdomen, and by the agency of their terminal hooks or holders. Such observations seem to afford presumptive evidence of the parasitic nature of these animals.

It does not appear to me probable that the *Ocythoë* ascends to the surface of the water by exhausting its shell of the included water; for if this were the fact, those females whose shell is in great part filled with eggs, could not visit the surface. But the change of specific gravity is doubtless effected in its own body, by which it is enabled to sustain itself on the surface at will, or to descend to the bottom promptly at the approach of danger.

The shells which in structure and appearance approach nearest to *argonauta*, are unquestionably to be found in the PTEROPODA; and the examination of *Carinaria*, *Atlanta* and *Spiratella*, would almost lead us to suppose, that the artificer of *argonauta* is in reality of that division; but if this supposition be indicated by the conformation of the shell, it does

not seem to be corroborated by the probable habits of the animal. All those hitherto discovered of that group, are known to swim at the surface of the ocean, and not being furnished with other organs of locomotion than fins, they cannot glide upon the bottom; we must therefore (analogically) suppose this to have been the habit of the animal; and yet it is hardly admissible that it should, in that case, have eluded the observation of voyagers, since the shell has not unfrequently been found in a state of occupancy by the parasite.

VIII. *On Irregularities observed in the direction of the Compass Needles of H. M. S. Isabella and Alexander, in their late Voyage of Discovery, and caused by the attraction of the iron contained in the Ships.* By *Captain* Edward Sabine, *of the Royal Regiment of Artillery, F. R. S. &c.*

Read February 18, 1819.

It is proposed in this paper to show in what respects the effects of local attraction, in the above mentioned ships, were conformable to the observations which had been made in preceding voyages; and how far the errors, which were found to take place on different courses, and under different dips of the magnetic needle, corresponded with the rules for calculating corrections, which Captain Flinders had found useful in his own experience, and which he had recommended for a more extensive trial.

It may be desirable to premise, that the irregularities here alluded to, are not those accidental disturbances which may be caused by iron placed inadvertently too near the compasses; but the permanent, and constant effect of the mass of iron contained in a ship, affecting its compasses at all times, and in a greater or less degree, according as its influence is more or less powerful, in comparison to the directive force of the earth's magnetism. That errors have always existed from this cause, may be inferred, from the uncertainty which experience has attached to the results of azimuths observed in ships. The cause, however, appears to have been very long

unsuspected, whilst its effects had produced a general impression, that the azimuth compass was in itself an imperfect instrument, and only to be relied on within certain undefined and variable limits.

It was reserved to the accurate observation, and the habit of recording and comparing apparently trivial and accidental differences in results, which distinguished the late Mr. WALES (astronomer in the second voyage of Captain COOK,) to enable him to lead the way to a knowledge of the nature and causes of these errors; he remarks, "that in the passage of the Resolution and Adventure to the Cape of Good Hope, and subsequently, the greatest west variations had happened when the ship's head was north and easterly, and the least when it was south and westerly, differing very materially from one another with the ship's head in different positions, and still more when observed in different ships;" thus manifesting that they were something more than accidental.

This voyage was the last in which Mr. WALES embarked, and the investigation does not appear to have been pursued in this country until the voyage of discovery to Terra Australis, in the first years of the present century. The survey of the coast of New Holland being carried on in a considerable measure, by the intersection of compass bearings taken from the deck of the Investigator, so much embarrassment and perplexity were found to arise from the effects of local attraction, that much of Captain FLINDERS's attention and thoughts were necessarily devoted, to a consideration of some means of remedying the inconvenience.

On his return to England, he obtained permission from the Lords Commissioners of the Admiralty to make a course of

experiments in ships under their direction at the principal sea ports, with a view to ascertain if compasses were similarly affected in other ships, and to try the general applicability of rules which he had found useful in correcting the errors in the Investigator. These rules, with the observations and reasonings on which they were founded, were published in a short paper in the Philosophical Transactions, and in a more detailed form in Appendix No. II. in the Voyage to Terra Australis. There are three points in these statements chiefly worthy of attention, from their practical importance; and on which it seems desirable, therefore, to notice how far his observations have been confirmed by those made in the Isabella and Alexander.

FIRST; he found that in every ship a compass would differ very materially from itself, on being removed from one part of the ship to another. Experience of this source of irregularity, had induced him early in his voyage, to confine the use of the compass, with which his survey was carried on, to one particular spot. The place he selected was determined by convenience in other respects; it was on the binnacle, and exactly amidships.

The Isabella and Alexander had not completed half their voyage across the Atlantic, before it was found that the binnacle compasses of the one ship differed very materially, in indicating the course steered, from those of the other: namely, one point, or $11\frac{1}{4}°$. No dependance whatsoever could be placed on the agreement of compasses in different parts of the ship, or of the same compass with *itself*, if removed but a few inches: even in the neighbourhood of the binnacles the variation, as observed amidships, was from 8° to 10°

greater than the result of azimuths taken by a compass placed between two or three feet on the larboard side; and an almost equal difference in a contrary direction took place on removing the compass to the starboard side, rendering it a matter of some trouble and difficulty, to make the azimuth compass agree with those in the binnacle by which the ship was steered, and for which it was therefore necessary to determine the variation.

As the ships ascended Davis's Strait, these latter compasses began to traverse so sluggishly, that it was necessary to shake the binnacles continually to assist their motion. The cards of these* had a metal rim round their circumference, weighing one ounce, eleven drams avoirdupois, which, as the directive power of magnetism diminished, became too heavy for the needle to carry round: they were also frequently found to disagree with each other from $\frac{1}{4}$ or $\frac{3}{4}$ of a point; the consequence, most probably, of the different local attraction to which they were exposed. These compasses ceased therefore to be attended to, except as an occasional assistance to the helmsman, and a position was selected in each ship, in which a compass on a more suitable construction was permanently fixed; by this the ship's course was directed, azimuths taken, and bearings of land, &c. noted, during the voyage.

This *standard compass*, as it may be called, was placed in the Isabella exactly amidships, between the main and mizen mast, on a stout cross beam elevated nine or ten feet above the deck; this beam was the usual walk of the Greenland pilot, or of the quarter master, as affording a better view of the ice among which the ships were frequently steered, than

* Burt's patent binnacle compasses.

from the deck. The elevation was an advantage to the compass in such high magnetic latitudes, by rendering it less liable to accidental disturbance, on the removal of such implements of iron as were required to be kept on deck for use. The Alexander not having a similar cross beam, her compass was fixed amidships on a box of sand placed on the companion, between five and six feet above the deck.

SECONDLY; Captain FLINDERS found that in his compass permanently fixed as described, no error took place when the ship's head was on the magnetic north or south points; showing, that at such times the attraction of the ship, and of magnetism, were in the same line of direction. The maximum of error also took place when the ship's head was at right-angles to these points; namely, at east or west; being however in opposite directions, in excess of the true variation on the one side, and in defect on the other; so that the extreme difference occasioned by altering the course from east to west or the reverse, would be twice the error at either.

On the intermediate points, the ratio of the error to its maximum was as the "sine of the angle between the ship's head and the magnetic meridian to the sine of eight points or radius," or sufficiently near to admit of corrections being calculated for every course, when the error on a single one was known by observation.

Thus far the experiments which Captain FLINDERS tried in every ship corresponded, excepting only that the maximum of error in different ships at the same place would differ materially.

The accordance in so many ships gave him reason to believe that in compasses placed near the binnacle, and amidships, *the points of no error* would be most commonly

those of the magnetic meridian. Considering, however, that this must depend altogether on the distribution of iron, and may be therefore liable to great diversity, he recommends, that, in every ship, as soon as a fixed position has been selected for a compass, the points of no error should be determined by repeated observation. The method that was adopted for this purpose in the late voyage appearing both simple and effectual, it may be useful to exemplify it by an instance or two.

The Isabella being at anchor in Brassa Sound, Shetland, her head was placed, by means of warps, on each point of the compass successively, and the bearing of a pile of stones on the summit of a distant hill noted by her compass at each point; at the same time that these observations were made on board, her bearing from the hill was also observed by a compass placed on the pile of stones; the *agreement* in bearing showed the points of no error, and the *differences* the errors in each point, without the calculation which azimuths involve. [See Pl. X. fig. 1.]

The Alexander being along side a floe of ice in Baffin's Bay, the true magnetic bearing from the ship, of a very distant and well defined object on the main land, was found by carrying a compass on the ice in an opposite direction, to a distance which insured its being perfectly free from local influence. The ship's head being then warped round to each point of the compass successively, the errors in each were determined by the difference in bearing, as in the last instance. [See Pl. X. fig. 2.]

The regularity in the above results is the best testimony that the method is a satisfactory one. Certain precautions must be attended to: thus, the object must be sufficiently

distant, that the space occupied in warping the ship round may not subtend any sensible parallax. The direction of the ship's head should be noted by the compass by which the bearings are taken. A short time must be allowed to elapse after the ship is steady on any point, to ensure the traversing of the cards: this is particularly necessary in high latitudes when the compasses move very sluggishly. And lastly, the observations should be repeated.

It will be observed by the above results, in the Isabella and Alexander, that the points of no error were not coincident in either ship with those of the magnetic meridian; in the Alexander especially, they were more nearly at right angles to it. That this ship should have differed so materially from all the instances on record, may be attributed to her compass being so near the level of the deck, and therefore being more affected by the influence of a considerable quantity of iron articles (such as ice anchors, ice saws, &c.) which were carried on the after part of the deck for convenience in use, than it would have been, had it been raised higher. This was proved by placing a compass on a plank elevated for experiment in front of the companion, to the same height as in the Isabella, namely, nine or ten feet. The points of no error were found, in this position, to be about north and south, and the amount of error at eight points, nearly twenty degrees; the same as in the Isabella: the greatest error at the same time, by the Alexander's standard compass, viz. the one nearer the decks, being 8° 20′ at N. N. E. The dip was 84° 09′.

The propriety of Captain FLINDERS's recommendation, to determine the points of no error in a fixed compass by actual

observation in every ship as soon as the distribution of iron is completed, may therefore be considered as confirmed by the observations in the Isabella and Alexander; whilst his rule of proportion may receive a verbal alteration to render it more suitable for general application: so corrected, it would be as follows. The expressions substituted being marked in italics, and the original words entered in the margin.

East or west. Magnetic meridian.

"The error produced in any direction of the ship's head, will be to the error at *the point of the greatest irregularity*, as the sine of the angle between the ship's head and *the points of no error* to the sine of eight points or radius."

THIRDLY; Capt. FLINDERS's experience in the Investigator showed that the maximum of error in the same compass, would be different in different parts of the world, although the use of the compass was confined to one particular spot in the ship, and every precaution taken to avoid an interference with the distribution of the ship's iron.

It is worthy of remark, that by multiplying observations and by comparing the series one with another, he was thus *practically* led to trace a connection between the amount of the errors and the dip of the needle; a knowledge of the fact preceding, in his mind, any theoretic suggestion that such might be the case.

It does not appear indeed that the principal cause of this connection was even subsequently known to him; he perceived that the influence of local attraction on the compass needle increased as the dip became greater. He endeavoured to account for this circumstance, on a supposition that all iron

might receive an *absolute* increase in the intensity of its attractive power by approaching the magnetic pole.

The increase, however, which was the subject of his observation, was a *relative* one, being in comparison to the directive power of magnetism. A diminution in the latter would therefore produce the effect equally with an absolute augmentation in the former; and that such a diminution does take place, and in a degree which is sufficient to account for all the effects observed, will be evident to every person, who reflects that although the magnetic force is greatest at the pole, its directive power must then have wholly ceased, having become less on the horizontal traversing of the needle in proportion as the point of attraction has been brought beneath the compass; indicated by the angle which the dipping needle makes with the horizon. This is doubtless the principal cause of the connection which Captain FLINDERS was the first to trace.

It is not designed to say that this cause may not be aided by the increased magnetism of portions of the ship's iron, such as bars and stanchions; which being fixed in an upright position, may receive an addition to their attractive power where the position of the dipping needle is always coincident with theirs; but merely to observe, that a cause is known to exist for the connection, independently of supposition; which cause, conjointly with experience, shows the inadequacy of the rule proposed by Captain FLINDERS, whereby the amount of error, under any known dip, being ascertained, the amount may be calculated for any other dip, by using as a multiplier, the decimal expression of the proportion which

the error, in the one ascertained instance, may have borne to the dip.

In the observations made in the Isabella at Shetland, where the dip is 74° 21½′, the maximum of error was 5° 34′ easterly of the true variation, with the ship's head at E. S. E. and 5° 46′ westerly at W. N. W. making an extreme difference of 11° 20′.

By Captain FLINDERS's rule, the common multiplier for this compass would have been about one twelfth, or .083, which at a dip of 86° 09′, which was the greatest observed during the late voyage, would have given an error of between 7° and 8°, making the extreme difference 15°; whereas repeated observation showed it to be at that time more nearly 50°, if not exceeding that amount.

The inadequacy of the rule will also appear by reference to the observations made by the Alexander in Baffin's Bay. The error at eight points being 6° 46′, at a dip of 84° 30′; it ought scarcely to have exceeded 7° at the greatest possible dip, making an extreme difference of less than 15°. No opportunity occurred indeed of making accurate observations at a greater dip than the above; but the difference in the bearing of objects before and after tacking indicated with sufficient certainty, that the error had increased to an amount very far beyond 15°; frequent instances of an extreme difference of from 3 to 4 points being remarked, as the ship approached the farthest western longitude to which she attained in a high latitude; this was in Lancaster's Sound of Baffin, into which inlet the expedition sailed beyond the 81° of west longitude in the parallel of 74° and a few minutes.

It is much to be regretted that the service did not admit an opportunity to be afforded, of making observations on the

various magnetic phenomena, with the excellent instruments supplied to the expedition, at this very interesting place; where a nearer approach was made to one of the magnetic poles than had ever been known before.

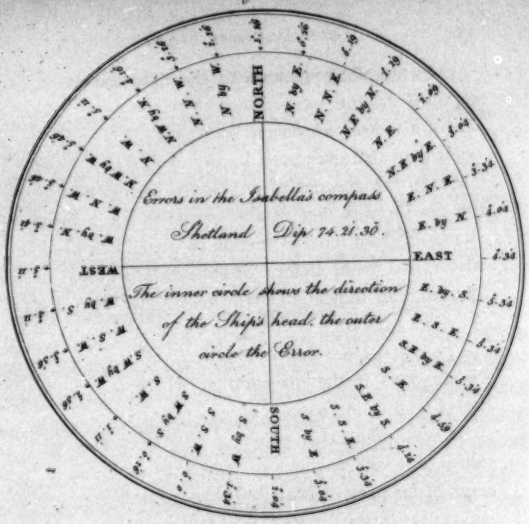
But in the absence of any actual observation on the dip of the needle, this fact of the error of the compasses having increased from local attraction so greatly beyond the amount which had been before observed, is worthy of notice, as affording an indication that the dip had also increased, and not inconsiderably. The greatest which was observed, was 86° 09′; and after this observation, the ships continued to sail for six days in the direction in which the dip had hitherto been found to increase.

In concluding this paper, it may be permitted to remark, that it is to the voyages of discovery undertaken during the reign of his present Majesty, that a knowledge of the extent and causes of the errors to which a compass is subject in ships, is to be principally attributed; as well as the steps that have been taken towards the investigation and remedy of the inconvenience they occasion to practical navigation.

The care and exertions of Captain FLINDERS in collecting observations for this purpose, give his opinions and rules a peculiar claim to attentive consideration. No one could have been more fully persuaded than he was, that a rule, founded on the effects experienced in a few ships, would require a far more extensive trial, before it could be depended on for general application.

To carry this on, therefore, is to follow his useful example, and to effect what he was desirous to have done himself, had his life been spared.

Fig. 1.


NORTH
EAST
SOUTH
WEST
Errors in the Isabella's compass
Shetland Dip. 74. 21. 30.
The inner circle shows the direction of the Ship's head, the outer circle the Error.

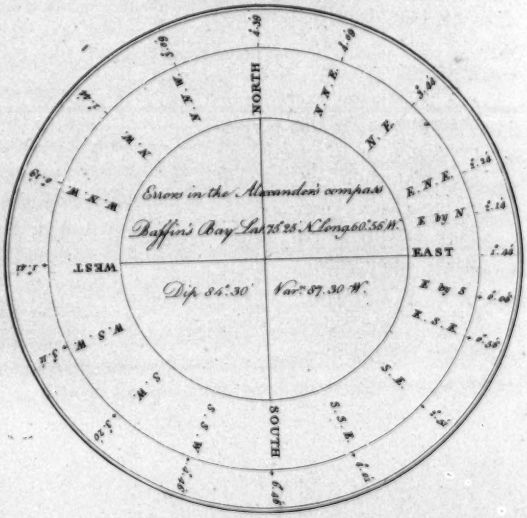

NORTH
EAST
SOUTH
WEST
Errors in the Alexander's compass
Baffin's Bay Lat. 75. 25. N. Long. 60. 56. W.
Dip. 84. 30. Var. 87. 30 W.

Fig. 2.

IX. *Some observations on the formation of Mists in particular situations.* By Sir H. Davy, *Bart. F.R.S. V.P.R.I.*

Read February 25, 1819.

ALL persons who have been accustomed to the observation of Nature, must have frequently witnessed the formation of mists over the beds of rivers and lakes in calm and clear weather after sun set; and whoever has considered these phenomena in relation to the radiation and communication of heat and nature of vapour, since the publication of the researches of M. M. RUMFORD, LESLIE, DALTON, and WELLS, can hardly have failed to discover the true cause of them. As, however, I am not aware that any work has yet been published in which this cause is fully discussed, and as it involves rather complicated principles, I shall make no apology for offering a few remarks on the subject to the Royal Society.

As soon as the sun has disappeared from any part of the globe, the surface begins to lose heat by radiation, and in greater proportions as the sky is clearer; but the land and water are cooled by this operation in a very different manner: the impression of cooling on the land is limited to the surface, and very slowly transmitted to the interior; whereas in water above 45° FAHRENHEIT, as soon as the upper stratum is cooled, whether by radiation or evaporation, it sinks in the mass of fluid, and its place is supplied by warmer water from below, and till the temperature of the whole mass is reduced

nearly to 40° F. the surface cannot be the coolest part. It follows, therefore, that wherever water exists in considerable masses, and has a temperature nearly equal to that of the land, or only a few degrees below it, and above 45° F. at sunset, its surface during the night, in calm and clear weather, will be warmer than that of the contiguous land; and the air above the land will necessarily be colder than that above the water; and when they both contain their due proportion of aqueous vapour, and the situation of the ground is such as to permit the cold air from the land to mix with the warmer air above the water, mist or fog will be the result; which will be so much the greater in quantity, as the land surrounding or inclosing the water is higher, the water deeper, and the temperature of the water, which will coincide with the quantity or strength of vapour in the air above it, greater.

I shall detail some observations which appear to me to show the correctness of this view. June 9th, 10th, 11th, the temperature of the atmosphere and of the Danube was repeatedly examined during a voyage that I made upon this river from Ratisbonne to Vienna, and on each of these days, the sky being perfectly clear, the appearance of mist above the river in the evening uniformly coincided with the diminution of the temperature of the air from three to six degrees *below* that of the river, and the disappearance of fog in the morning with the elevation of the temperature of the air *above* that of the river. From Ratisbonne to Passau, the temperature of the Danube was pretty uniform throughout the 24 hours, being highest, 62° F. or 62¼° F., between 12 and 2 o'clock, and about one degree less before sun-rise, and the temperature of the air from 61° F. to 73° F. during the day,

and from 61° to 54° F. during the night. Below Passau, the Inn and the Ilz flow into the Danube.* On examining the temperature of these rivers at 6 o'clock, A. M. June 11, that of the Danube was found to be 62° F., that of the Inn $56\frac{1}{2}$° F., and that of the Ilz 56° F.: the temperature of the atmosphere on the banks where their streams mixed, was 54°. The whole surface of the Danube was covered with a thick fog; on the Inn there was a slight mist, and on the Ilz barely a haziness, indicating the deposition of a very small quantity of water. About 100 yards below the place where the three rivers joined, the temperature of the central part of the Danube was 59° F., and here the quantity of mist was less than on the bed of the Danube before the junction; but about half a mile below, the warmer water had again found its place at the surface, and the mist was as copious as before the union of the three rivers. June 18th, the evening was cloudy, and the temperature of the atmosphere remained till after dark higher than that of the river, being, when the last observation was made, 63° F. when there was not the slightest appearance of mist. The sky was clearer before sun-rise on the 19th, and the thermometer immediately after sun-rise, in the air above the river, stood at $55\frac{1}{2}$° F. the temperature of the Danube being 61° F.; a thin mist was seen immediately above the river; but there being no mass of vapour to exclude the sun-beams, it rapidly disappeared, and was not visible a few feet from the surface; and in half an hour the whole atmosphere was perfectly transparent.

In passing along the Rhine from Cologne to Coblentz,

* The Danube was greenish, the Inn had a milky blueness, the Ilz was perfectly pellucid; but from the rapidity with which the Inn descended, its waters at this spot give their tint to the whole surface.

May 31st and June 2d and 3d, the nights being very clear, the same phenomenon of the formation of mists was observed, precisely under the same circumstances; but as I could examine the temperature of the air and of the river only close to the banks, and in two or three situations, my observations were less precise and less numerous; the mist formed later in the evening, and disappeared sooner in the morning than on the Danube; which was evidently owing to the circumstances of the atmosphere being warmer and the river colder, the temperature of the one being from 66° F. to 75° F. during the day, and that of the river, where I examined it, from 59° to 60° F.

July 11th. I examined the temperature of the Raab near Kermond in Hungary, at 7 o'clock, P. M. and found it 65° F. that of the atmosphere being 72° F. During the whole evening there were some thin fleecy clouds in the western sky, which being lighted up by the setting sun, greatly interfered with the cooling by radiation from the earth, and at half past nine the thermometer, in the atmosphere, was still 69° F. and at half past ten 67° F., when there was not the slightest appearance of mist. In the morning, before sun-rise, the temperature of the atmosphere on the banks was 61° F., that of the river 64° F., and now the bed of the river was filled with a white thin mist, which entirely disappeared half an hour after sun-rise.

I made similar observations on the Save in Carniola, in the end of August; on the Izonzo in the Friul, in the middle of September; on the Po near Ferrara, in the end of September; and repeatedly on the Tiber and on the small lakes in the Campagna of Rome in the beginning of October; and I have never in any instance observed the formation of mist on a

river or lake, when the temperature of the water has been lower than that of the atmosphere, even when the atmosphere was saturated with vapour.

It might at first view be supposed, that whether the cooling cause existed in the water or the land, the same consequences ought to result; but the peculiar properties of water, to which I referred in the beginning of the paper, render this impossible. Water in abstracting heat from the atmosphere becomes lighter, and the warmer stratum rests on the surface, and its operation in cooling the atmosphere is extremely slow; besides, the cooled atmospheric stratum remains in contact with it, and water cannot be deposited from vapour, when that vapour is rising into an atmosphere of a higher temperature than its own; and the law holds good, however great the difference of temperature. Thus, August 26th, at sun-set, the day after a heavy fall of rain, and when the atmosphere was exceedingly moist, I ascertained the temperature of the Drave near Spital in Carinthia, and though it was 14° F. below that of the air, yet the atmosphere above the river was perfectly transparent.

It may be imagined, that without any reference to the cooling agencies of air from the land, mist may form upon rivers and lakes, merely from the loss of heat by radiation from the air, or the vapour itself immediately above the water; and that the phenomenon is merely one of the formation of vapour, the source of heat being in the water; and its deposition, the source of cold, being in the atmosphere; but it is extremely improbable, that air or invisible vapour, at common temperatures, can lose any considerable quantity of heat by radiation; and, if mist could be formed from such a

source, it must always be produced to a great extent upon the ocean in calm weather during the night, particularly under the line, and between the tropics, which the journals of voyages sufficiently prove is not the case. I have myself had an opportunity of making some observations which coincide with this view. During a voyage to and from Pola, I passed the nights of September 3, 5, and 6, off the coast of Istria; there was very little wind on either of the nights, and from sun-set till nearly midnight it was perfectly calm in all of them. On the 3d it was cloudy, and the lightning was perceived from a distant thunder storm, and the vessel was never far from the shore: but on the 5th and 6th the sky was perfectly clear, and the zodiacal light, after sun-set, wonderfully distinct and brilliant, particularly on the 5th, and we passed by the help of oars from two to eight miles from the shore. The temperature of the sea at sun-set was 76° F. on the 5th, 77° F. on the 6th, that of the atmosphere immediately above it 78° F. and 79° F. On the 5th, at midnight, about five miles from the shore, the temperature of the sea was 74° F. and that of the atmosphere 75° F., and on the 6th, at the same hour, at about four miles from the shore, the temperature of the sea was 73° F. and that of the atmosphere 75° F. There was not the slightest appearance of mist on either of these nights on the open sea, or at any distance from the land: but close under the hills of Istria there was a slight line of haze visible before sun-rise, which was thickest under the highest land; and as we approached at sun-rise, on the 7th, the mountains of the Friul, the tops of those nearest to Trieste were seen rising out of a thick white mist, which did not reach a quarter of a mile from the shore.

After mists have formed above rivers and lakes, their increase seems not only to depend upon the constant operation of the cause which originally produced them, but likewise upon the radiation of heat from the superficial particles of water composing the mist; which produces a descending current of cold air in the very body of the mist, whilst the warm water continually sends up vapour: it is to these circumstances, that the phenomena must be ascribed of mists from a river or lake, sometimes arising considerably above the surrounding hills. I have often witnessed this appearance during the month of October, after very still and very clear nights, in the Campagna of Rome above the Tiber, and on Monte Albano over the lakes existing in the ancient craters of this extinguished volcano, and, in one instance, on the 17th of October, before sun-rise, there not being a breath of wind, a dense white cloud of a pyramidal form was seen on the site of Alban lake, and rising far above the highest peak of the mountain, its form gradually changed after sun-rise, its apex first disappeared, and its body, as it were, melted away in the sun beams.

Where rivers rise from great sources in the interior of rocks or strata, as they have the mean temperature of the climate, mists can rarely form upon them except in winter, or late in autumn, or early in spring. In passing across the Apennines, October 1st, 2d, and 3d, 1818, there having been much rain for some days preceding, and the nights being very clear, I observed the beds of all the rivers in the valleys filled with mist, morning and evening, except that of the Clitumnus near its source, in which there was no mist, and this river rises at once from a lime-stone bed, and when I examined it,

at half past six o'clock, A. M. October 8, was $7\frac{1}{2}^{\circ}$ lower than the atmosphere.

Great dryness in the air, or a current of dry air passing across a river, will prevent the formation of mist, even when the temperature of the water is much higher than that of the atmosphere: thus on the 14th of June, near Mautern, though the Danube at five in the morning was 61° F. and the air only 54°, yet there was no mist; but a strong easterly wind blew, and from the rapidity with which water evaporated it, it was evident that this wind was in a state of extreme dryness.

The Tiber has furnished me with a number of still more striking examples. October 13th, the night having been very clear, on arriving at the Ponte Molle, at half past six in the morning, I found no mist on the river, yet the temperature of the air immediately above it was 48° F. and that of the river 56° F., a strong north wind blew, which indicated, by the hygrometer, a degree of dryness of 55°, and this part of the river was exposed to it; but the valley above, where the river was sheltered from the wind, was full of mist, and the mist in rising to the exposed level might be seen, as it were, dissolving, presenting thin striæ which never reached above a certain elevation, and many of which disappeared a few seconds after they rose. From the 13th to the 25th of Oct. during which time the tramontane or north wind blew, I witnessed repeatedly the same phenomenon, and in the whole of this time there was only one morning when there was no mist in the sheltered valleys, and the cause was perfectly obvious; the night had been very cloudy, and the thermometer, before sun rise, indicated a difference of only one degree in the atmosphere below that of the river.

It is not my intention to discuss the general subject of the deposition of water from the atmosphere, in this paper; but merely to describe a local cause of considerable extent and variety in its modifications: and which is not without an effect in the economy of nature, for verdure and fertility, in hot climates, generally follow the courses of rivers, and by the operation of this cause, are extended to the hills, and even to the plains surrounding their banks.

Rome, Dec. 8, 1818.

X. *Observations on the Dip and Variation of the Magnetic Needle, and on the Intensity of the Magnetic Force; made during the late voyage in search of a North West Passage.* By Captain Edward Sabine, *of the Royal Regiment of Artillery, F. R. S. and F. L. S.*

Read February 25, 1819.

THE dipping needle used in these observations is the property of HENRY BROWNE, Esq.; it was made by Messrs. NAIRNE and BLUNT, and is similar in construction to one made by the same artists, and described by the Hon. HENRY CAVENDISH in the 66th volume of the Philosophical Transactions, as used in the house of the Royal Society.

Previously to delivering it into my charge, Mr. BROWNE had adjusted the balance of the needle by means of the screws on the cross of wires attached to its axis; so that no alteration took place in the indication of the dip, on reversing the poles.

The instrument was placed in the direction of the magnetic meridian, by a compass stationed at a sufficient distance, and suffered to remain during the observations for the purpose of occasional verification. When time admitted, the correctness of adjustment was also proved, by observing the minimum of dip. An equal number of observations were made with the face of the instrument towards the east and towards the west; the arcs indicated at both ends of the needle were read.

In determining the intensity of the magnetic force, the needle was drawn to an horizontal position by a magnet, and being released at an observed moment of time, was suffered

to oscillate until the arcs became too small to be readily distinguished: the first arc was thus equal to the dip, and at every tenth vibration both the arc and time were noted. The observations in the magnetic meridian were repeated with the face of the instrument towards the east and towards the west.

It is highly satisfactory to notice the agreement of the results which were obtained in London and in Shetland, at different periods, and by different observers; showing that the adjustment of the balance of the needle was preserved during the voyage, notwithstanding the accidents to which it was liable: and as a testimony of the excellence of the instrument, and of the confidence which may be placed in observations made with it.

Observations on the dip.

1818.	Latitude.	Longitude.	No. of obser.	Observer.	Dip.	Remarks.
	° ′	° ′			° ′ ″	
April 13	51 31 N.	0 08 W.	16	Capt. Kater	70 34 39	Regent's Park, London.
30	60 09½	1 12	14	Capt. Sabine	74 22 48	Brassa Island, Shetland.
May 1	60 09½	1 12	12	Lieut. Parry	74 20 10	Brassa Island, Shetland.
June 9	68 22	53 50	12	Capt. Sabine	*83 08 07	On ice.
19	70 26	54 52	14	Capt. Sabine	*82 48 47	Hare Island.
July 8	74 04	57 52	10	Capt. Sabine	84 09 15	(Baffins) three Islands.
23	75 05	60 03	10	Lieut. Parry	84 24 57	On ice.
23	75 05	60 03	10	Capt. Sabine	84 25 15	On ice.
Aug. 2	75 51½	63 06	10	Capt. Sabine	84 44 30	On ice.
4	75 59	64 47	10	Capt. Sabine	84 52 06	On ice.
19	76 32	73 45	10	Capt. Sabine	85 44 23	On ice.
20	76 45	76 00	14	Lieut. Parry	86 08 53	On ice.
20	76 45	76 00	14	Capt. Sabine	86 09 33	On ice.
25	76 08	78 29	16	Capt. Sabine	85 59 31	On ice.
Sept. 11	70 35	66 55	10	Capt. Sabine	84 39 21	On ice.
Nov. 3	60 09½	1 12		Lieut. Parry	74 21 06	Brassa Island, Shetland.
3	60 09½	1 12	14	Capt. Sabine	74 21 47 15	Brassa Island, Shetland.
1819.						
March	51 31	0 08	16	Capt. Sabine	70 33 16	Regent's Park, London.

It is probable that the needle was affected by local attraction either on the 9th or on the 19th of June; but on which day it is difficult to say. On the 9th the ships were anchored to an iceberg of very considerable size, on which the observations were made, the instrument being removed as far as possible from the ships. On the 19th it was used in the observatory which was erected on Hare Island; every fastening of this ingenious and useful building was of brass, and the greatest care was taken to prevent the needle being disturbed by local or accidental causes. But there were several basaltic columns on the face of a hill which rose immediately from the observatory, which may have had an influence; as these columns on Hare Island are said, by Professor GIESECKE,* to have a powerful effect on the needle.

Observations on the intensity of the magnetic force.

Regent's Park, London, April, 1818. *By Captain* KATER.

Perpendicular to the meridian.

			Account of vibrations.	
100	8 21,6			' "
100	8 15			0 59
				0 43
				0 30
				0 21
				0 15
				0 10
				0 6
				0 4
				0 1
100	8 18,3	Mean	No. of vibrations.	
				0
				20
				30
				40
				50
				60
				70
				80
				90
				100

The subsequent observations were made by Captain SABINE.

* Art. Greenland, BREWSTER'S Cyclopædia.

Brassa Island, Shetland, lat. 60° 09′, *long.* 1° 12′ *W.*

In the magnetic meridian.

Number of vibrations.	Interval.		Time.	Seconds.
	m. s.			
100	7 49·5			
100	7 50			
100	7 49·75	Mean		

Arc.	No. of vibration.	Seconds.
74°	0	50
53	10	48·5
40	20	47
30	30	46
22	40	46
16	50	46
12	60	47
7	70	47
5	80	47
3	90	46
1	100	46

Perpendicular to the meridian.

Number of vibrations.	Interval.		Time.	Seconds.
	m. s.			
100	8 00			
100	7 59			
100	7 59·5	Mean		

Arc.	No. of vibrations.	Seconds.
90°	0	52
57	10	49
42	20	47
32	30	48
24	40	48
18	50	48
13	60	47
9	70	47
5	80	47·5
3	90	47·5
1	100	47·5

On an iceberg in Davis's Strait, lat. 68° 22′, *long.* 53° 50′ *W.*

In the magnetic meridian.

Number of vibrations.	Interval.		Time.	Seconds.
	m. s.			
100	7 17			
100	7 23			
100	7 20	Mean		

Arc.	No. of vibrations.	Seconds.
83°	0	48
59	10	45
45	20	45
35	30	45
27	40	43
21	50	44
16	60	43
12	70	44
9	80	43
7	90	44
4	100	44

Perpendicular to the meridian.

Number of vibrations.	Interval.		Time.	Seconds.
	m. s.			
100	7 33			

Arc.	No. of vibrations.	Seconds.
90°	0	47
60	10	46
40	20	45
35	30	45
27	40	42
21	50	45
16	60	44
12	70	51
8	80	44
6	90	44
4	100	

On Hare Island, lat. 70° 26′, *long.* 54° 52′ *W.*

In the magnetic meridian.

Number of vibrations.	Interval.			Time.	Seconds.
	m. s.				48
100	7 22			Arc.	83° 59
100	7 24				48
100	7 21	Mean			36
					27
					21
					16
					12
					9
					6
					4
				No. of vibrations.	0
					10
					20
					30
					40
					50
					60
					70
					80
					90
					100

Perpendicular to the meridian.

Number of vibrations.	Interval.			Time.	Seconds.
	m. s.				48
100	7 26			Arc.	90° 62
					47
					37
					28
					22
					17
					13
					9
					7
					5
				No. of vibrations.	0
					10
					20
					30
					40
					50
					60
					70
					80
					90
					100

On ice in Baffin's Bay, lat. 75° 05′, *long.* 60° 28′.

In the magnetic meridian.

Number of vibrations.	Interval.			Time.	Seconds.
	m. s.				47·5
100	7 29			Arc.	84° 59
100	7 25·5				45
100	7 27·5	Mean			35
					26
					19
					14
					10
					7
					4
					1
				No. of vibrations.	0
					10
					20
					30
					40
					50
					60
					70
					80
					90
					100

Perpendicular to the meridian.

Number of vibrations.	Interval.			Time.	Seconds.
	m. s.				48
100	7 26			Arc.	90° 61
					46
					36
					28
					21
					16
					12
					9
					6
					4
				No. of vibrations.	0
					10
					20
					30
					40
					50
					60
					70
					80
					90
					100

On ice in Baffin's Bay, lat 75° 51½′, *long.* 68° 06′ *W.*

In the magnetic meridian.

Number of vibrations.	Interval.		Time.	Seconds.
	m. s.		Arc.	84° 58 46 35 28 22 17 13 10 7 5
100	7 21,5			
100	7 25			
100	7 23,25	Mean	No. of vibrations.	0 10 20 30 40 50 60 70 80 90 100

Perpendicular to the meridian.

Number of vibrations.	Interval.		Time.	
			Arc.	
			No. of vibrations.	0 10 20 30 40 50 60 70 80 90 100

On ice in Baffin's Bay, lat. 76° 45′, *long.* 76° *W.*

In the magnetic meridian.

Number of vibrations.	Interval.		Time.	Seconds. 47 45 44 43 43 43 42 44 43 43
	m. s.		Arc.	86° 60 46 36 28 22 18 13 10 7½ 5
100	7 13			
100	7 17			
100	7 15	Mean	No. of vibrations.	0 10 20 30 40 50 60 70 80 90 100

Perpendicular to the meridian.

Number of vibrations.	Interval.		Time.	Seconds. 44 44 44 43 43 43 44 43 44 44
	m. s.		Arc.	90° 65 45 35 27 21 17 13 9½ 7 5 3½
100	7 16			
			No. of vibrations.	0 10 20 30 40 50 60 70 80 90 100

On ice in Baffin's Bay, lat. 76° 08½′, *long.* 78° 21′ *W.*

In the magnetic meridian.

Number of vibrations.	Interval.		Time.	Seconds.
	m. s.		Arc.	85° 51 40 31 24 19 14 11 8 6 4
100	7 15			
100	7 17			
100	7 16	Mean	No. of vibrations.	0 10 20 30 40 50 60 70 80 90 100
			Time.	46 45 44.5 43.5 44 43 43 43 43 44.5 44.5

Perpendicular to the meridian.

Number of vibrations.	Interval.		Time.	Seconds.
	m. s.		Arc.	90° 57 48 35 27 21 17 13 10 7 5
100	7 18			
			No. of vibrations.	0 10 20 30 40 50 60 70 80 90 100
			Time.	46.5 45 44.5 43.5 43.5 43.5 43 43.5 43.5

On an iceberg in Davis's Straits, lat. 70° 33′, *long.* 66° 55′ *W.*

In the magnetic meridian.

Number of vibrations.	Interval.		Time.	Seconds.
	m. s.		Arc.	85° 47 36 28 21 17 12 9 6 4 3
100	7 16			
100	7 16			
100	7 16	Mean	No. of vibrations.	0 10 20 30 40 50 60 70 80 90 100
			Time.	46 44 44 43 43 43.5 43.5 43 42

Perpendicular to the meridian.

Number of vibrations.	Interval.		Time.	Seconds.
	m. s.		Arc.	90° 80 67 57 50 43 37 32 27 23 19 15 12 9 7
100	7 18,5			
			No. of vibrations.	0 10 20 30 40 50 60 70 80 90 100
			Time.	46 45.5 44 44.5 43.5 43.5 43.5 43.5 43 41.5

1819, *March.* *In the Regent's Park, London, lat.* 51° 31′ 40″, *long.* 0° 08′ *W.*

In the magnetic meridian.

Number of vibrations.	Interval.		Time.	Seconds.
	m. s.		Arc	71° 55 40 30 23 17 13 9 6 3 1 —
100	8 01,5			
100	8 08			
100	7 56,5		No. of vibrations.	0 10 20 30 40 50 60 70 80 90 100
100	8 02	Mean		

Perpendicular to the meridian.

Number of vibrations.	Interval.		Time.	Seconds. 52 51 49,5 49,5 49 48,5 49,5 48,5 50,5 49,5
	m. s.		Arc.	90° 55 40 30 22 16 11 7 4 2 1 —
100	8 18,5			
			No. of vibrations.	0 10 20 30 40 50 60 70 80 90 100

Abstract of the times in which 100 *vibrations were performed.*

Latitude. N.	Longitude. W.	In the meridian.	First Arc.	Perpendicular to the meridian.	First Arc.	
° ′	° ′	m. s.		m. s.	°	
51 31	0 08	0 0	0	8 18,3	90	Regent's Park, London.
60 09	1 12	7 49½	74	7 59,5	90	Shetland.
68 22	53 50	7 20	83	7 33	90	On ice, Davis's Straits.
70 26	54 52	7 21	83	7 20	90	Hare Island.
75 05	60 23	7 27½	84	7 26	90	On ice, Baffin's Bay.
75 51½	63 06	7 23½	84	0 0	—	On ice, Baffin's Bay.
76 45	76 00	7 15	85	7 26	90	On ice, Baffin's Bay.
76 08	78 21	7 16	85	7 18	90	On ice, Baffin's Bay.
70 35	66 55	7 16	83	7 18,5	90	On ice, Davis's Straits.
51 31	0 08	8 02	70	8 18	90	Regent's Park, London.

The 100th vibration never exceeded an arc of 3°

Observations to determine the variation of the needle, in Davis's Strait, and Baffin's Bay.

The azimuth compasses used in these observations were constructed on an improved plan, the invention of Captain KATER. It is thus described in the "Instructions for the use of the Instruments furnished to the Northern Expeditions," printed by order of the Royal Society:

"The compass is five inches diameter; by means of an inclined mirror and lenses, the degrees are seen by reflection considerably magnified; a line drawn on a piece of ivory is viewed at the same time, and serves as an index by which the degrees are to be read off.

"At the opposite side of the box is a sight on which slides, in a frame, the segment of a glass cylinder, ground to a radius of five inches. By means of this, a fine line of light is thrown on the index, and may be seen at the same time as the degrees on the card.

"The degrees on the card are read from the north towards the east, and are carried round to 360°, in order to obviate the possibility of error in this respect."

The observations were made either on shore or on the ice, sufficiently distant from the ship to be beyond the influence of her attraction. The compass was placed on a copper fastened stool, and was carefully levelled by means of a spirit level, to ensure the perpendicularity of the sight vane.

Each altitude and azimuth is a mean of several observations, the compass being removed and levelled afresh between every one, thus making each faithfully distinct.

The mean Greenwich time is given, as it determines the amount of the sun's polar distance.

The latitudes and longitudes are of the spot, deduced by the ship's log from the nearest observed.

The altitudes are corrected for index error only, the letters or signs annexed denote the limb, and whether by reflection or by the natural horizon.

The observed azimuths are of the sun's centre cleared of index error; the compasses used were No. 1 and 2, supplied to the Isabella, and No. 3 to the Alexander; the true azimuths deduced from the elements contained in the preceding columns, are expressed in a corresponding manner to the reading of the compass, for the purpose of comparison.

The observations were made on ice, except when otherwise noted in the column of remarks.

When due consideration is given to the greatly diminished power, with which the earth's magnetism acts on the horizontal direction of the needle, when the dip becomes so considerable as it was found in Davis's Straits and Baffin's Bay, namely, from 83° to 86°; the satisfactory results which have been obtained, even under such extreme circumstances with Captain KATER's compasses, afford the best testimony of their excellence, and of the precision which may be expected from them in the ordinary course of observation.

It may also be remarked, that a difference in the result of azimuths observed at different hours of the day may not be altogether an error of observation, since it is probable that as the directive power of magnetism diminished, the causes which produce the hourly change in the variation itself may act with increased effect.

Should the amount of this change be considerably augmented in high magnetic latitudes, careful observations on the direction of the needle at different hours of the day, on all convenient occasions, might be serviceable towards a more certain knowledge of its causes, than has been hitherto obtained from observations made where the effects are so inconsiderable.

The influence of the ship's iron on their compasses increasing, as the directive power of magnetism diminished, produced irregularities that rendered observations on board ship of little or no value towards a knowledge of the true variation; a few azimuths which were observed in the Isabella, have been selected for the purpose of exemplifying this remark. They will also show, how essential it is to navigation in high latitudes, that the nature of the errors which the ship's attraction produces in her compasses, should be understood.

Observations to determine the variation of the needle, made on shore, or on the ice. Observer, Captain SABINE.

1818.	Mean Greenwich time.			Latitude.		Longitude.		Observed Altitude.		Observed azimuth.		Compass.	True azimuth.		Variation.		Remarks.
	h.	m.	s.	°	′	°	′	°	′	°	′		°	′	°	′	
June 9	22	0	0	68	23½ N.	53	47 W.	48	23 15 ☉	156	27	1	88	55½	67	31½ W.	
11	23	20	0	68	14	54	15	63	20 58 ☉	175	47	2	108	07	67	40	
12	0	0	0	68	14	54	15	70	02 33 ☉	186	16	1	118	12	68	04	
17	8	20	0	70	26½	54	52	56	15 36 ☉	332	54	1	260	08	72	46	
17	8	30	0	70	26½	54	52	55	32 34 ☉	334	05	1	261	11½	72	53½	
18	8	28	0	70	26½	54	52	55	20 52 ☉	332	52	1	261	33	71	19	Observatory, Hare Island.
18	8	35	0	70	26½	54	52	55	33 45 ☼	334	24	1	263	10½	71	13½	
								53	57 35 ☉								
18	23	20	0	70	26½	54	52	64	43 18 ☉	184	35	1	112	55½	71	39½	
27	9	00	00	71	02½	54	13	50	18 13 ☉	343	26½	1	268	13½	75	13	
27	9	20	00	71	02½	54	13	49	18 20 ☼	346	33½	1	271	13	75	20½	
27	9	40	00	71	02½	54	13	47	07 0 ☼	349	54½	1	274	23	75	31	
27	10	00	00	71	02½	54	13	44	36 40 ☼	353	35	1	278	0	75	35	
27	10	20	00	71	02½	54	13	42	07 17 ☼	357	24½	1	281	36½	75	48	
July 4	10	45	00	72	44½	56	49	37	18 47 ☼	7	13½	1	288	18½	78	55	
6	9	12	0	73	22½	57	32	49	10 00 ☼	348	16	1	268	15	80	01	
12	23	30	0	74	01½	57	52	55	11 52 ☼	186	49½	1	106	05½	80	43½	On Baffin's three Islands.
21	10	05	0	74	58	59	16	38	49 57 ☉	0	33½	1	276	00½	84	33	
22	23	02	0	75	04	60	03	45	46 0 ☉	185	16	1	98	16½	86	59½	
28	10	20	0	75	28	60	34½	35	04 10 ☼	7	40	1	279	21½	88	18½	
30	8	17	0	75	32	61	0	48	23 57 ☉	336	24½	1	248	46½	87	37½	
30	10	20	0	75	32	61	0	36	29 35 ☉	0	55½	1	272	41½	88	13½	
Aug. 2	10	00	00	75	44½	64	0	17	33 15 L.	2	26	1	273	25	89	01	
2	10	10	00	75	44½	64	0	17	01 25 L.	4	31	1	275	37½	88	53½	
4	0	12	30	75	59	64	32	46	39 55 ☉	204	01	1	113	43½	90	17½	
6	9	34	0	70	50½	64	34	37	27 20 ☉	354	24	1	263	16½	91	07½	
12	0	00	51	75	54½	65	30	40	19 34 ☉	202	31½	1	109	45½	92	45½	
12	0	12	51	75	54½	65	30	40	59 55 ☉	205	24½	2	110	50½	94	34½	
12	0	29	30	75	54½	65	30	43	36 28 ☉	210	52½	1 & 2	117	12½	93	39½	
19	2	13	00	76	30	72	35	45	59 50 ☉	240	32	1 & 2	138	23	102	09	
19	2	33	00	76	30	72	35	47	27 10 ☉	246	53½	2	143	37½	103	16	Compasses used alternately.
19	2	33	00	76	30	72	35	47	33 40 ☉	245	58½	1	144	03½	101	55	
22	9	00	00	76	32½	76	52½	36	54 40 ☉	348	56	1	241	00	107	56	
25	8	27	10	76	08½	78	21	40	32 30 ☼	341	05½	2	230	07	110	58½	Compasses used alternately.
25	8	27	10	76	08½	78	21	40	14 05 ☼	341	17½	1	230	58	110	19½	
25	10	03	00	76	08½	78	21	30	06 47 ☼	4	05	1	254	56	109	09	Observed by the silk line, the line of light not being perceptible owing to the weather.
25	10	08	00	76	08½	78	21	28	25 25 ☉	5	43½	3	256	18	109	25½	
Sept. 11	8	30	00	70	35½	66	55½	13	27 30 L.	330	44	1 & 2	244	06	86	38	Dip of horizon allowed for 51 feet 7 in. measured.
11	8	36	00	70	35½	66	55½	12	59 47 L.	332	58½	2	245	36	87	22½	Compasses used alternately.
11	8	40	00	70	35½	66	55½	12	39 33 L.	333	19½	1	246	41	86	38½	

In the column of "observed altitude," ☉ signifies the lower, and ☼ the upper limb of the sun, the altitude being taken by reflection; L, the lower limb by the natural horizon.

Results of azimuths observed on board the Isabella, with WALKER'S *azimuth compass, placed amidships in front of the companion.*

1818.	Latitude.	Longitude.	Ship's head.	Variation.	Remarks.
	° ′	° ′		° ′	
June 3	65 38	54 24	NW. by W.	67 10	
4	65 47	54 44	N.	66 22	
4	65 47	54 44	W.	77 34	
4	65 46	54 44	E. S. E.	47 54	
5	65 47	54 22	N.W.	76 47	
5	65 47	54 22	N.	67 32	
7	65 50	55 0	NE. by E.	49 57	
27	74 02	54 13	E.	64 56	
27	74 02	54 13	S. E.	67 07	
27	74 02	54 13	S.	76 27	
27	74 02	54 13	S.W.	84 38	True variation observed on the ice 75° 30′.
27	74 02	54 13	W.	93 16	
27	74 02	54 13	N.W.	90 20	
27	74 02	54 13	N.	77 44	
27	74 02	54 13	N. E.	70 30	
Aug. 29	74 44	77 50	W.N.W.	128 35	
30	74 21	78 0	N. by E.	104 28	
Sep. 3	73 55	73 47	N. by W. ½ W.	108 55	
4	73 23	75 58	E. by S. ½ S.	85 04	
5	72 34	74 06	S.E.	74 22	
5	72 34	74 06	S. ½ W.	90 32	
5	72 34	74 06	S. S. E.	78 50	
11	70 37	66 58	N.W.	98 42	
11	70 37	66 58	N.W. by W.	100 08	True variation observed on the ice 86° 53′.
11	70 37	66 58	W.N.W.	100 56	
11	70 37	66 58	W. by N.	99 36	
16	68 40	64 0	N. by E.	73 56	
16	68 40	64 0	S.E. by E. ½ E.	59 07	
29	64 53	62 10	N.N.W.	71 21	
29	65 02	62 10	S.W.	75 40	
Oct. 14	59 30	36 07	E. S. E.	39 04	
14	59 30	36 07	N.W. by N.	52 55	

XI. *On the action of crystallized surfaces upon light.* *By* David Brewster, *LL. D. F. R. S. Lond. and Edin. In a letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G. C. B. P. R. S. &c. &c. &c.*

Read February 25, 1819.

My dear Sir,

It has been remarked by Malus, in his Theory of Double Refraction, "that the action which the first surface of Iceland "spar exercises upon light, is independent of the position of its "principal section;—that its reflecting power extends beyond "the limits of the polarising forces of the crystal, and that as "light is only polarised by penetrating the surface, the forces "which produce extraordinary refraction begin to act only at "this limit." He also observes, that "the angle of incidence "at which Iceland spar polarises light by partial reflection, is "56° 30′; that it then comports itself like a common transpa-"rent body; and that whatever be the angle comprehended "between the plane of incidence and the principal section of "the crystal, the ray reflected by the first surface is always "polarised in the same manner." *

These conclusions, obtained experimentally by an author of such distinguished eminence, I should naturally have received as established truths, had I not been led, by a series of experiments made before the perusal of his work, to opinions of an opposite kind. My experiments indicated an extension of the polarising forces *beyond* the crystal; and I

* *Theorie de la Double Refraction*, pp. 240. 241.

was thus induced to question the accuracy of MALUS's views, and to repeat the experiments upon which he had founded them. The results of this investigation, while they have overturned the opinions hitherto adopted, have at the same time led to the establishment of several points both of theoretical and practical importance.

In giving an account of these results, I shall *first* consider the effects produced upon transmitted light by a change in the mechanical condition of the surfaces of crystals, and then establish the laws according to which the interior forces affect and modify the forces which produce partial reflection.

SECT. I. *On the effects produced upon transmitted light, by a change in the mechanical condition of the surfaces of crystals.*

If we take a hexaedral prism of *nitrate of potash*, and observe a luminous object through two of its inclined surfaces that have a good natural or artificial polish, we shall perceive two distinct and perfectly formed images. If we now roughen these two surfaces, and cement upon each of them a plate of glass by means of *balsam of capivi*, the character of the two images will be greatly changed. The image that has suffered the greatest refraction will be as distinct as before, but the other image will be either of a faint reddish colour, or wholly invisible, according to the degree of roughness induced upon the refracting surfaces. When *oil of cassia* is used instead of the balsam, the least refracted image, if it was visible before, will now be completely extinguished.

By substituting pure *alcohol*, or the *white of an egg*, instead of the balsam, the least refracted image will become distinct, and the most refracted image will be either a mass of nebulous

light, or almost invisible. A result nearly similar will be obtained with water, notwithstanding its effect in dissolving the little prominences which constitute the superficial roughness of the crystal.

In order to explain these phenomena, we must recollect that the index of refraction for the ordinary image of nitre is 1.511, and that of the extraordinary image 1.398. When the rough surface of the nitre is covered with balsam of capivi, which has nearly the same index of refraction as the ordinary image, the same effect is produced as if the rough surface had been polished for the ordinary rays. All the little pits or depressions in the rough surface being filled up with balsam, the ordinary rays suffer little or no refraction in penetrating the crystal, and therefore the image which they form will be as clear and distinct as in the first experiment. But since the index of refraction for the extraordinary image is much less than that of the balsam, the rays of which it is composed will not enter the crystal undisturbed, but will be scattered in the same manner as if its surface was rough, and had a refractive power corresponding to the difference between the index of refraction for the extraordinary ray, and the index of refraction for the balsam. When water or alcohol is substituted in room of the balsam, the effects now described are interchanged, the roughness being removed for the extraordinary rays by the application of a fluid of the same refractive density, while the rays that form the ordinary image are dispersed by the refractions which still exist at the rough surface of the crystal.

These effects will be still better understood by supposing the crystal to consist of an extraordinary and an ordinary

medium, arranged in alternate strata. When the superficial polish of both these media is removed, the application of the *balsam* restores, as it were, the polish of the ordinary medium, without restoring that of the extraordinary medium; while the application of the *alcohol* restores the polish of the extraordinary medium without restoring that of the ordinary medium.

When the refractive power of the fluid is intermediate between that of the two media, the ordinary and the extraordinary image will be equally indistinct; and we have it in our power to alter the distinctness of either of the images, by varying the refractive force of the interposed fluid.

If the plane of incidence is not perpendicular to the axis, a variation in the angle of incidence will produce a variation in the index of refraction of the extraordinary ray; and, since the refractive power of the interposed fluid suffers no change, the extraordinary image must become more or less distinct, according as its index of refraction is made to approach more or less to that of the fluid, by varying the inclination of the refracted ray to the axis.

From the preceding experiments, which have been repeated with the same results with *calcareous spar*, *arragonite*, and many other crystals, we may deduce the following conclusions:

1. The force of double refraction and polarisation extends not only without the interior limit of the ordinary refractive force, but also without the surface of the crystal.

2. The force of double refraction and polarisation emanates from the surface of bodies, though its intensity depends upon the inclination of the surface to the axis of the crystal.

3. The ordinary or the extraordinary image may be ex-

tinguished at pleasure in any doubly refracting crystal; and the crystal is thus converted into a singly refracting crystal, like certain specimens of agate.

4. In soft crystals that do not admit of a perfect polish, the distinctness of any of the two images may be made a maximum, by giving the crystal the best polish of which it is susceptible, and then cementing plates of glass upon its surfaces, by a transparent cement of the same refractive power as that of the pencil which is to be rendered most distinct. If it is required to make the two images equally distinct, the refractive power of the cement must be a mean between that of the ordinary refraction, and the extraordinary refraction which corresponds to the angle which the refracted ray forms with the axis of double refraction.

5. All doubly refracting crystals consist of an ordinary and an extraordinary medium, alternating with each other, and varying in density according to a law which I have described in another paper.*

I consider the optical structure of *agate* as demonstrating the existence of two media. In quartz, the two media are equally perfect and transparent; but in certain specimens of agate, the one medium is seen in a separate state from the other, and broken down into small portions like the figures 333. The light which passes through these portions, is evidently acted upon by a different refractive power from that which

* The paper here alluded to, was laid before the Royal Society of Edinburgh on the 16th of March, 1818; but as it could not have been understood without the preceding experiments, its publication was necessarily delayed. The theory which it contains embraces also the complex phenomena which arise from the combination of two or more axes. See the *Phil. Trans. Lond.* 1818, p. 264.

passes through the rest of the crystal, and is polarised in a transverse plain.*

SECT. II. *On the influence of the polarising force of doubly refracting crystals, upon the polarising force which accompanies partial reflection.*

The experiments in the preceding section could not fail to throw a doubt upon the identity of action exercised upon reflected light by crystallized and uncrystallized surfaces. I was therefore led to a more minute investigation of the subject, and obtained a series of very unexpected results, which I shall explain under the three heads into which they naturally arrange themselves.

1. *On the change produced upon the polarising angle by the interior forces of doubly refracting crystals.*

In order to examine with care the superficial action of calcareous spar, I exposed several surfaces by cleavage, and having selected the one that had the most perfect polish, I covered all the other sides of the rhomb with black wax, and measured the polarising angles in planes variously inclined to the principal section. The following are the results of a great number of observations:

Position of the crystal.	Azimuth.	Polarising angle.	No. of observations.
Short diagonal in plane of reflection	0°	57° 14′	39
One of the edges in plane of reflection	50	57½′ 58 3	15
Long diagonal in plane of reflection	90	59 3	37
Difference between the greatest and least polarising angle - - - -		2° 18′	

* See *Phil. Trans.* 1813, p. 104; 1814, p. 191; and *Edin. Trans.* vol. vii. p. 298, 9.

The following observations were made with rhombs taken from a different mass of calcareous spar:

Position of the crystal.	Azimuth.	Polarising angle.	No. of observations.
Short diagonal in plane of reflection	0°	57° 36′	5
One of the edges in plane of reflection	50	57½′ 58 50	7
Long diagonal in plane of reflection	90	59 44	7

In these experiments the results were the same, whether the obtuse angle of the rhomb was nearest or farthest from the eye, or whether it was to the right or left hand of the observer.

In order to determine the angle of polarisation for surfaces differently inclined to the axis, I selected some fine Faroë crystals of calcareous spar from the cabinets of Sir GEORGE MACKENZIE and Mr. ALLAN. One of these crystals was an acute dodecahedron, having a highly polished surface inclined about 5° to the axis, and with it I obtained the following results.

Position of the crystal.	Polarising angle.	No. of observations.
Axis in plane of reflection -	54° 18′	13
Axis perpendicular to plane of reflection - - -	58 14	10

I was now desirous to obtain a surface perpendicular to the axis; but I have searched in vain for such a specimen, and have invariably found that the summit of the prism is rough and unpolished. With a surface polished by art, and cut perpendicular to the axis, I found the polarising angle about 58° 15′; but I do not regard this result as deserving any particular attention.

In order to supply the defect of natural faces, I ground and polished a great variety of surfaces, inclined at all angles

to the axis; but the results clearly proved that the peculiar action of the surfaces, in varying the polarising angle, is exhibited only by the highly polished faces which are sometimes obtained from cleavage, or which occur in perfect crystals.

The following observations were made with a fine crystal of *Chromate of lead*:

Position of the crystal.	Polarising angle.	No. of observations.
Axis of prism in plane of reflection	67° 48′	4
Axis of prism perpendicular to the plane of reflection	65 42	4

In the first of these positions a great quantity of brilliant blue light remained unpolarised, whereas in the second position the whole of the pencil suffered complete polarisation.

2. *On the change produced upon the polarisation of the reflected ray, by the interior forces of doubly refracting crystals.*

Since the extraordinary force in calcareous spar was thus shown to extend to such a distance beyond the surface as to modify the polarising angle produced by superficial reflection, it became extremely probable that the polarisation of the reflected ray might suffer some change from the same cause: but after the most careful observation, I could not discover the slightest indication of such an effect. Upon reflecting farther, however, on the nature of the change which I had expected, it occurred to me that the action of the ordinary reflecting force was so powerful, as to mask the influence of the inferior force which emanated from the axis, and that the effect of the one might be rendered visible by diminishing the intensity of the other. I accordingly introduced a film of oil of Cassia between a glass prism and the surface of the spar,

and having inclined the prism at a very small angle to that surface, I thus separated the image formed at the common surface of the prism, and the oil from the image formed at the common surface of the oil and the spar. The effect was exactly what I had anticipated. The influence of the ordinary reflecting force was reduced almost to nothing, and the light reflected from the separating surface of the oil and the spar, was polarised at an angle of about $45\frac{1}{2}°$, and was almost entirely under the dominion of the force which emanated from the axis. The following were the results obtained with an ordinary surface, inclined $45° 29\frac{1}{2}'$ to the axis.

1. *Azimuth* 0°. When the plane of the principal section is in the plane of reflection, the light reflected at the surface of the oil and the spar is polarised in the plane of reflection, the obtuse solid angle being farthest from the eye. The light of the image is of a faint red colour, and has very little intensity.

2. *Azimuth* 12°. The obtuse angle being farthest from the eye, the reflected pencil is polarised about 45° out of the plane of reflection.

3. *Azimuth* 42°. The reflected pencil is polarised transverse to the plane of reflection, or 90° out of it. The light is now of a yellowish white tint, and is much more intense than in azimuth 0°.

4. *Azimuth* 90°. When the plane of reflection is perpendicular to the plane of the principal section, the obtuse solid angle being either to the right or left hand, the reflected pencil is polarised a little more than 135°, or −45° out of the plane of reflection. The intensity of the pencil is now intermediate between that of azimuth 0° and 45°.

5. *Azimuth* 180°. The obtuse angle being now next the

eye, the pencil is polarised 180° out of the plane of reflection, or it has again returned into that plane.

In passing through the last 45° of azimuth, the polarisation varies very slowly, the change being only about 10°; whereas in passing through the first 45° of azimuth, the polarisation varies no less than 90°, indicating in the most unequivocal manner, as we shall afterwards see, that this change depends upon the angle which the incident ray forms with the axis of the crystal.

The light reflected from the separating surface of the oil and the spar is a *maximum*, when the plane of the principal section is perpendicular to the plane of reflection, and its colour is then nearly white. When these two planes coincide, the intensity of the light is a *minimum*, and its colour is then a faint red; and in intermediate positions, the reflected pencil has both its intensity and its colour of an intermediate character. In the azimuth of 45°, the reflected pencil exhibits a very curious phenomenon when analysed with calcareous spar. Its colour is then yellowish white, and all the *yellow* light is polarised transversely to the plane of reflection. One of the images, however, instead of vanishing, consists of *blue* and *red* light, the *red* vanishing, and the *blue* becoming more brilliant as the analysing prism is turned to the *left*; and the *blue* vanishing, and the *red* becoming more brilliant as the prism is turned to the *right*. This effect arises from the difference in the angles at which the red and blue rays are incident upon the separating surface of the oil and the spar. Each set of rays, therefore, as will afterwards appear, suffers a different change of polarisation, the one being polarised about 87° out of the plane of reflection, and the other 93°.

I have repeated the preceding experiments by substituting in place of oil of cassia, *water*, *alcohol*, *castor oil*, *balsam of capivi*, and *oil of anise seeds*, a series of fluids whose refractive powers increase progressively. With *water*, the light refuses to be polarised completely in the direction of the long diagonal, while it suffers complete polarisation in the direction of the short diagonal. With *alcohol*, the direction of the polarisation is not altered. With *castor oil*, the intensity of the light is greater in the direction of the long diagonal, than in that of the short one; and in the former case, the pencil is polarised at a much greater angle than in the latter. With *balsam of capivi*, in the azimuth of 45°, the pencil is polarised about 15° out of the plane of reflection. In the azimuth of 90°, the pencil is not completely polarised at any angle, but is nearly so in the plane of reflection, and at a considerable angle of incidence. In 0° of azimuth, the pencil is completely polarised in the plane of reflection. With *oil of anise seeds*, in azimuth 45°, the pencil is polarised about 45° out of the plane of reflection. In azimuth 90°, the pencil refuses to be polarised at any angle, and in 0° of azimuth, the polarisation is complete in the plane of reflection.

As the preceding results were obtained with a surface inclined 45° 28½′ to the axis, I was anxious to observe the effects produced by the Faroë crystals, where the natural faces are nearly in the plane of the axis. I accordingly repeated the experiments with a variety of these crystals, and in every case I observed the same phenomena. In the azimuth of 90°, where the polarising angle is 58° 14′, the pencil was polarised a degree or two out of the plane of reflection. In the azimuth of 45°, where the polarising angle is about

36° 16′, the change of polarisation is about 40°; and in the azimuth of 0°, where the polarising angle is 54° 18′, the change of polarisation was a little more than 90° when the obtuse angle was farthest from the eye, and about 106° when the obtuse angle was nearest the eye. In all these positions the image reflected from the surface of the oil and the spar, is nearly as bright as that from the surface of the prism and the oil.

In order to determine the change of polarisation when the plane of reflection was perpendicular to the axis, it was necessary to have a prismatic crystal of calcareous spar with a polished summit; but I have always found this summit rough and unpolished. There was therefore no alternative but to polish an artificial face cut in this direction; and upon the application of oil of cassia, I found that in every azimuth the change of polarisation was about 75°. The colour of the image was a bright yellow, and a little blue light remained at the point of evanescence.

In extending these experiments to other crystals I have obtained similar results; but there are none so well fitted for this species of examination as calcareous spar. In applying oil of cassia to a very fine prism of chromate of lead, the direction of the polarisation was not in the slightest degree altered, as the ordinary action was not sufficiently weakened to render visible the influence of the interior force. When the plane of reflection passed through the axis of the prism, *blue* light remained in the vanishing image; but in a plane rectangular to this, the light was completely polarised, as in the experiment when the reflecting surface was in contact with air.

I now tried *rock crystal* and *oil of anise seeds*, which have nearly the same mean refraction; but on account of the great debility of the interior polarising force, it was not able to overpower or even to modify that which accompanies partial reflection. I could easily have reduced this last force still farther till it came under the dominion of the first; but the reflecting power would have been reduced in the same proportion, and would not have been capable of driving back a number of rays sufficient to form a perceptible image. But though the polarisation is not changed at the separating surface of the oil and the rock crystal, yet the character of the reflected light is modified in a very remarkable manner. When the plane of reflection from one of the sides of the prism of rock crystal was in the direction of the axis, or in 0° of azimuth, the reflected image was a *deep blue* of very little intensity; whereas in a rectangular direction, where the azimuth was 90°, it was of a brick red colour, and much more luminous. On one of the faces of the pyramid, in azimuth 0°, the tint was a *brilliant pink*, intermediate between the *red* and the *blue*; and on the same face, in 90° of azimuth, it was of a *brick red* colour as before. These variations are obviously related to the axis of double refraction, and indicate the extension of its force within the sphere of partial reflection. The origin of the colours themselves, I shall soon have occasion to explain, in a paper on the action of uncrystallized surfaces.*

3. *General results deduced from the preceding experiments.*

Had it been in my power to command a series of the most

* This Paper was read before the Royal Society of Edinburgh, on the 4th January 1819.

perfect crystals, or to communicate to artificial faces that high polish which nature often exhibits, I might have obtained a more complete generalisation of the preceding phenomena. Limited, however, as the investigation has been by these causes, it still presents us with several views of great generality and interest.

First. The force of double refraction and polarisation extends without the surface of crystals, and within the sphere of the force which produces partial reflection.

Second. The change in the angle of polarisation produced by the interior force, depends on the inclination of the reflecting surface to the axis of the crystal, and also on the azimuthal angle which the plane of reflection forms with the principal section.

In any given surface, where A and A″ are the minimum and maximum polarising angles, viz. in the azimuth of 0° and 90°, the polarising angle A′ at any intermediate azimuth a, may be found by the formula

$$A' = A + \text{Sin.}^2 a\,(A'' - A).$$

In the rhomboidal surfaces of calcareous spar

$$A'' - A = 138'.$$

Third. The change in the direction of the polarisation must be produced after the ray has suffered reflection; for if the change preceded reflection, the reflecting force would have polarised it in the plane of reflection, whatever had been the direction of its previous polarisation.

Fourth. The change in the direction of the polarisation depends upon the angle which the incident ray forms with the axis of the crystal, and takes place in such a manner that if

φ = angle of incident ray with the axis; and

C = change in the direction of the polarisation,

we shall have

$$\text{Sin. } \tfrac{1}{2}\,C = \sqrt{\text{Sin. } \varphi}.$$

If we make

A = complement of the inclination of the reflecting plane to the axis;

α = azimuth of the plane of incidence with the principal section; and

i = angle of incidence reckoned from the perpendicular,

we shall have

$$\text{Cos. } \alpha \times \text{Tang. } A = \text{Tang. } z, \text{ and}$$

$$\text{Cos. } \varphi = \frac{\text{Cos. } A \times \text{Cos. } (i \pm z)}{\text{Cos. } z}.$$

In one of the ordinary rhomboidal surfaces where the inclination to the axis is 45° 28½′, A = 44° 36½′; and with oil of cassia i or the incidence of the mean ray, when the polarisation is complete, is about 46° 17′. I have assumed it at 45° 28½′ (which will be more correct for the mean luminous ray than 45° 17′) for the purpose of making the change of polarisation commence with zero in 0° of azimuth.

Upon these principles I have computed the following table, which shows the change in the direction of the polarisation, corresponding to any azimuth and any inclination of the incident ray with the axis.

TABLE *showing the change in the direction of the polarisation in different azimuths.*

Azimuth.	Inclination of incident ray to the axis.	Change in the direction of the polarisation.
°	° ′	° ′
0	0 0	0 0
10	6 54	40 36
20	16 50	65 6
30	23 0	77 22
40	29 24	88 52
45	32 38	94 34
50 57½	36 29	100 54
60	42 17	110 10
70	48 31	120 0
80	54 37	129 8
90	60 0	137 0
100	65 56	145 48
110	71 4	153 4
120	75 42	159 48
129 2½	79 28	165 3
135	81 41	169 46
140	83 22	170 34
150	86 12	174 52
160	88 18	177 36
170	89 33	179 28
180	90 0	180 0

The results in the preceding table enable us to explain the phenomenon described in p. 154. As the interposed oil of cassia has a prismatic form and a very high dispersive power, the blue and the red rays are incident at different angles with the axis, and therefore the change in the direction of their polarisation must be different. The nearest approximation to evanescence in one of the images, belongs to the mean ray of the spectrum, and therefore at this point the image that should have vanished, must consist of blue and red light, one of which will disappear *before*, and the other *after*, the mean ray.

I have the honour to be, &c. &c. &c.

DAVID BREWSTER.

To the Right Hon. Sir JOSEPH BANKS, Bart. G.C.B. P.R.S. &c. &c. &c.

Edinburgh, Nov. 12, 1818.

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Postscript to Dr. YOUNG's *Letter on the Reduction of Experiments.* P. 96.

5. *Corrections for Refraction.*

1. A simple and convenient method of calculating the precise magnitude of the atmospherical refraction, in the neighbourhood of the horizon, has generally been considered as almost unattainable; and Dr. BRINKLEY has even been disposed to assert the "impossibility of investigating an exact formula," notwithstanding the "striking specimens of mathematical skill, which," as he justly observes, "have been exhibited in the inquiry." We shall find, however, that the principal difficulties may be evaded, if not overcome, by some very easy expedients.

2. The distance from the centre of the earth being represented by x, and the weight of the superincumbent column by y, the actual density may be called z, and the element of y will vary as the element of x and as the density conjointly; consequently $dy = -mzdx$; the constant quantity m being the reciprocal of the modulus of elasticity. The refractive density may be called $1+pz$, p being a very small fraction; and it is easy to see that the perpendicular u, falling on the direction of the light, will always vary inversely as the refractive density, since that perpendicular continually represents the sines of the consecutive angles, belonging to each of the concentric surfaces, at which the refraction may be supposed to take place (Nat. Phil. II. p. 81:) and $u = \frac{s}{1+pz}$, s being a constant quantity. The angular refraction at each point will obviously be directly as the elementary change of this perpendicular, and inversely as the distance v from the point of incidence; whence the fluxion of the refraction will be $\frac{du}{v} = dr$, as is already well known.

3. For the fluent of this expression, which cannot be directly integrated, we may obtain a converging series by means of the TAYLORIAN theorem; but we must make the fluxion of the refraction constant, and that of the density variable; so that the equation will be $u = \frac{dv}{dr}\cdot r + \frac{d^2v}{dr^2}\cdot\frac{r^2}{2} + \frac{d^3v}{dr^3}\cdot\frac{r^3}{2.3} + \ldots$, v being the initial value of u, when $r = 0$. Now the whole variation, of which u is capable, while z decreases from $\frac{s}{1+p}$ to s; or, since p is very small, from $s - ps$ to s; and dr being $= \frac{du}{v}$, we have the equation $ps = vr + \frac{dv}{dr}\cdot\frac{r^2}{2} + \ldots$ But $v = \sqrt{(s^2 - u^2)}$, $dv = \frac{zdz - pdz}{v}$, and $\frac{dv}{dr} = \frac{z}{v}\cdot\frac{dz}{dr} - u$; and dz being $= -\frac{dy}{mz}$, and $du = -pzdz$, $\frac{dz}{dr} = \frac{v}{mpzz}\cdot\frac{dy}{dz}$.

4. We must now determine the value of the density z, which, when the temperature is uniform, becomes simply $= y$; but for which we must find some other function of y, including the variation of temperature; and we may adopt, for this purpose, the hypothesis lately advanced by Professor LESLIE, in the article Climate of the Encyclopædia Britannica, and suppose the density to be augmented, by the effect of cold, in the proportion of 1 to $1 + n\left(\frac{1}{z} - z\right)$, n being somewhat less than $\frac{1}{10}$; and since the density is as the pressure and the comparative specific gravity conjointly, we have $z = y\left(1 + n\left[\frac{1}{z} - z\right]\right)$, $\frac{z}{y} = 1 + \frac{n}{z} - nz$, $d\frac{z}{y} = \frac{dz}{y} - \frac{zdy}{yy} = -\frac{ndz}{zz} - ndz$, and $\frac{dy}{dz} = \frac{y}{z} + \frac{nyy}{z^3} + \frac{nyy}{z}$; consequently $\frac{dz}{dr} = \frac{v}{mpzz}\left(\frac{y}{z} + \frac{nyy}{z^3} + \frac{nyy}{z}\right)$ and $\frac{dv}{dr} = \frac{xy}{mpzzz} + \frac{nxyy}{mpzzz} + \frac{nxyy}{mpzz^3} - u$. We may proceed to take the next fluxion with respect to y, z, and v, the variations of u and x being comparatively inconsiderable: so that if we call $\frac{dv}{dr} = X + Y + Z - s$, its fluxion will be $X\left(\frac{dy}{ydr} - \frac{2dz}{zdr}\right) + Y\left(\frac{2dy}{ydr} - \frac{2dz}{zdr}\right) + Z\left(\frac{2dy}{ydr} - \frac{4dz}{zdr}\right)$: but since $\frac{dy}{ydr} = \frac{-v}{pzz} - \frac{2nvy}{pzz^3} - \frac{2nvy}{pzz}$, and $\frac{dz}{zdr} = \frac{-v}{pzz}$, we have $\frac{d^2v}{dr^2} = X\left(\frac{v}{pzz} - \frac{2nvy}{pzz^3} - \frac{2nvy}{pzz}\right) + Y\left(-\frac{2nvy}{pzz^3} - \frac{2nvy}{pzz}\right) + Z\left(\frac{2v}{pzz} - \frac{4nvy}{pzz^3} - \frac{4nvy}{pzz}\right) = \frac{vx}{mp^2z^5}\left(\frac{y}{z^3} - \frac{\text{“}3ny^2\text{”}}{z^5} - \frac{2ny^2}{z^3} - \frac{2n^2y^2}{z^3} - \frac{2n^2y^2}{z^3} + \right.$

$\frac{2xy^3}{z^5} - \frac{4x^2y^2}{z^6} + \frac{4x^3y^3}{z^7}\Big)$, or, initially $= \frac{v}{mp^3s^2}(1-2x-2x^2-2x^3-4x^4) = \frac{1-2x-12xx}{mp^3s^2}v$. In the next place, calling this fluxion H (K−L−M−N−P−Q) we obtain, for the fourth, H (K−L−M−N−P−Q) $\frac{dv}{ydr} + HK\left(\frac{dy}{ydr} - \frac{3dx}{xdr}\right) - HL\left(\frac{2dy}{ydr} - \frac{3dx}{xdr}\right) - HM\left(\frac{3dy}{ydr} - \frac{5dx}{xdr}\right) - HN\left(\frac{3dy}{ydr} - \frac{3dx}{xdr}\right) - HP\left(\frac{2dy}{ydr} - \frac{7dx}{xdr}\right) - HQ\left(\frac{2dy}{ydr} - \frac{5dx}{xdr}\right) = H(K-L-M-N-P-Q)\left(\frac{x}{mps}\left(\frac{y}{zz} + \frac{xyy}{zz} + \frac{xyy}{z^4}\right) - s\right) + HK\frac{v}{ps}\left(\frac{2}{x} - \frac{2xy}{z^3} - \frac{2xy}{z}\right) - HL\frac{v}{ps}\left(\frac{1}{x} - \frac{4xy}{z^3} - \frac{4xy}{z}\right) - HM\frac{v}{ps}\left(\frac{2}{x} - \frac{6xy}{z^3} - \frac{6xy}{z}\right) - HN\frac{v}{ps}\left(-\frac{6xy}{z^3} - \frac{6xy}{z}\right) - HP\frac{v}{ps}\left(\frac{5}{x} - \frac{4xy}{z^3} - \frac{4xy}{z}\right) - HQ\frac{v}{ps}\left(\frac{3}{x} - \frac{4xy}{z^3} - \frac{4xy}{z}\right) = \frac{x^2}{m^2p^2s^2}\left(\frac{y^2}{z^3} - \frac{2xy^3}{z^5} - \frac{2x^2y^4}{z^7} - \frac{2x^3y^5}{z^9} - \frac{4x^2y^3}{z^7} - \frac{4x^3y^4}{z^9} + \frac{xy^2}{z^5} - \frac{2x^2y^4}{z^7} - \frac{2x^3y^5}{z^9} - \frac{2x^2y^3}{z^5} - \frac{4x^3y^4}{z^7} + \frac{xy^2}{z^5} - \frac{2x^2y^4}{z^7} - \frac{2x^3y^5}{z^9} - \frac{2x^2y^3}{z^7} - \frac{4x^3y^4}{z^{9}} - \frac{4x^3y^4}{z^9}\right) - \frac{2}{mp^2s}\left(\frac{y}{z^3} - \frac{2xy^2}{z^5} - \frac{2x^2y^3}{z^7} - \frac{2x^2y^2}{z^5} - \frac{4x^3y^3}{z^7} - \frac{4x^3y^3}{z^7}\right) + \frac{v^2x}{mp^2s^2}\left(\frac{2y}{z^4} - \frac{2xy^2}{z^6} - \frac{2xy^2}{z^4} - \frac{2xy^2}{z^4} + \frac{8x^2y^3}{z^6} + \frac{8x^2y^3}{z^6} - \frac{4x^3y^3}{z^6} - \frac{4x^3y^3}{z^6} + \frac{12x^2y^4}{z^8} + \frac{12x^3y^4}{z^8} + \frac{12x^3y^4}{z^8} + \frac{12x^3y^4}{z^8} - \frac{20x^3y^5}{z^{10}} + \frac{16x^3y^5}{z^{10}} + \frac{16x^3y^5}{z^8} - \frac{12x^3y^4}{z^6} + \frac{16x^3y^5}{z^8} + \frac{16x^3y^5}{z^8}\right)$. It will be unnecessary to continue the whole series any further; but it will be satisfactory to obtain that part of the sixth term, which is independent of v; and for this purpose we must take the fluxion of the first part with respect to y and z, and then with respect to v; and that of the second twice with respect to v only; and it will be sufficient in this case to employ the initial values of $\frac{dy}{dr}$, $\frac{dz}{dr}$, and $\frac{dv}{dr}$, which are $\frac{-v(1+4x)}{ps}$, $\frac{-v}{ps}$, and $\frac{1+2x}{mps} - s$; and calling $1+4x = k$, the part required will be $\left(\frac{1}{m^3p^3s^6}(-2k+5+6kn-10n+8kn^2-14n^2+8kn^2-10n^2+12kn^2-36n^2+12kn^2-28n^2-3kn+5n+8kn^2-10n^2+10kn^2-14n^2+10kn^2-10n^2+16kn^2-36n^2+16kn^2-28n^2-3kn+7n+8kn^2-14n^2+10kn^2-18n^2+10kn^2-14n^2+16kn^2-44n^2+16kn^2-36n^2)-\frac{1}{mp^2s^2}[-k+3+4kn-6n+6kn^2-10n^2+6kn^2-6n^2+8kn^2-28n^2+8kn^2-20n^2]\right)\left(\frac{1+2x}{mps}-s\right)+2\left(\frac{1+2x}{mps}-s\right)^2\frac{1}{mp^2s^2}(2-2n-2n-2n+8n^2+8n^2-4n^2+12n^2+12n^2+12n^2+12n^2-20n^2+16n^2+16n^2-12n^2+16n^2+16n^2) = \left(\frac{1}{m^3p^3s^6}(3-6n-56n^2+128n^3+416n^4)-\frac{1}{mp^2s^2}[2-6n-20n^2+112n^2]\right)\left(\frac{1+2x}{mps}-s\right)+2\left(\frac{1+2x}{mps}-s\right)^2\frac{1}{mp^2s^2}.(2-6n-20n^2+112n^2)+\ldots$

6. The whole equation becomes therefore ultimately $ps = vr + \left(\frac{1+2x}{2mps} - \frac{s}{2}\right)r^2 + \frac{1-2x-12xx}{6mp^3s^2}vr^3 + \left(\frac{1-16n^2-24n^3}{24m^2p^3s^3} - \frac{1-2n-12nn}{24mp^3s} + \frac{2-6n-20n^2+112n^3}{24mp^3s^3}v^3\right)r^4 + \&c. + \left(\left[\frac{3-6n-56n^2+128n^3+416n^4}{720m^3p^3s^6} - \frac{2-6n-20n^2+112n^3}{720mp^3s^2}\right]\left(\frac{1+2x}{mps}-s\right)+2\left(\frac{1+2x}{mps}-s\right)^2\frac{2-6n-20n^2+112n^3}{720mp^3s^2}+\ldots\right)r^6+\ldots$ We also obtain, for finding, on this hypothesis, the height x, corresponding to the pressure y and the density z, the expression $mx - m = 1 - \frac{y}{z} + \frac{n}{q}\,\text{hl}\frac{2zz+qy(1-x)}{2zz-qy(1-x)}$; y being $= \frac{zx}{x+n-nzx}$, and $q^2 = 1+4n^2$. But the utility of the Taylorian theorem, thus applied, in obtaining a series, is not confined to Professor Leslie's hypothesis: it is equally well adapted to that of Laplace, or to any other admissible supposition respecting the distribution of temperatures: and we may therefore employ it in examining the comparative accuracy of the results of these different hypotheses.

7. Now if we take for n the value $\frac{45}{500} = .09$, corresponding to the multiplier 45, employed by Mr. Leslie, the refractions in the immediate neighbourhood of the horizon will become too great by about 1′; a difference by far too considerable to be attributed to the errors of observation only; and we must infer, that the law of temperature, obtained from the height of the line of congelation, is not correctly true, if applied to elevations remote from the earth's suface. Professor Bessel's approximation is also found to make the horizontal refraction too great. Mr. Laplace's formula, which affords a very correct determination of the refraction, is said to agree sufficiently well with direct observation also; but in fact this formula gives a depression considerably greater than was observed by Gay Lussac, in the only case which is adduced in its support; and the progressive depression follows a law which appears to be opposite to that of nature, the temperature varying less rapidly at greater than at smaller heights, while the observations of Humboldt and others seem to prove that in nature they vary more rapidly. Notwithstanding, therefore, the ingenuity, and even utility of Mr. Laplace's formula, it can only be considered as an optical hypothesis, and we are equally at liberty to employ any other hypothesis which represents the results with equal accuracy; or even to correct our formulas by comparison with astronomical observations only, without assigning the precise law of temperature implied by them. The theory will however afford us some general indications for this purpose; showing, for example, that the coefficient of the second term cannot be smaller than $\frac{1}{2mps} - \frac{1}{2}s$, whatever positive value we may attribute to n; and if we adjust the second and fourth coefficients, so as to represent the refractions near the zenith and at the horizon, without regarding the value of the subsequent terms, we shall obtain the third, by dividing the fourth by half of the second; since that part of the fourth coefficient, which occurs in the case of horizontal refraction, is always derived from the third by taking the fluxion with respect to v only, and is therefore found by multiplying the third by $\frac{dv}{4vdr}$, whatever the relations of the other quantities concerned may be.

8. On every supposition, the coefficient of the first term must be $\frac{v}{s}$, and that of the second must not greatly differ from $\frac{3}{s} - \frac{s}{2}$. The third coefficient, on the hypothesis of a law analogous to Mr. Leslie's, will be $1500\,\frac{v}{s}$; if we suppose the temperature to vary more uniformly, and make $x = y(1 + ix - i)$, the number will become 1900; or, taking $x = yx^2$, 2300, m being 766, and i 176: and Mr. Laplace's formula will of course give a value still larger. In fact the result of observation is represented with sufficient accuracy by the equation $.0002825 = v\frac{r}{s} + (2.5 + .5v^2)\frac{r^2}{s^2} + 3400\,v\frac{r^3}{s} + 3400\,(1.25 + .25\,v^2)\frac{r^4}{s^4}$, the barometer standing at 30 inches, and the thermometer at 50°: and this formula appears to be at least as accurate as the French tables. We have, for example:

Altitude	Refr.	Conn. d. T.	Altitude	Refr.	Conn. d. T.
° ′	′ ″	′ ″	° ′	′ ″	′ ″
0 0	33 52	33 52	20 0	2 39	2 40
5 0	9 57	9 56	30 0	1 41	1 41
10 0	5 21	5 21	45 0	58.15	58.3

The difference is somewhat greater a few degrees above the horizon; thus at 2° 17′ 50″, this formula makes the refraction 17′ 16″, the French tables 17′ 4″, Bradley's 17′ 30″, and Dr. Brinkley's observations reduced, 17′ 9″: but in such cases we can scarcely expect a greater degree of accuracy.

9. The terrestrial refraction may be most easily determined by an immediate comparison with the angle subtended at the earth's centre, the fluxion of which is $\frac{ud\,x}{vx}$, and $\frac{ud\,x}{vrdr}$ is initially the first part of the coefficient of the second term of the series already obtained, and is equal to 6; so that this angle, while it remains small, is six times the refraction: commonly, however, the refraction in the neighbourhood of the earth's surface is somewhat less than in this proportion.

10. The effects of barometrical and thermometrical changes may be deduced from the fluxion of the equation, if we make m, p, and n, or rather i, vary: and for this purpose it will be convenient to employ the form

$p_1 = vr + \left(\frac{1}{2(m-t)p} + \frac{r}{2}\right) r^2$, the value of the fraction, if we neglect the subsequent terms, becoming 3.41; and this expression is sufficiently accurate for calculating the whole refraction, except for altitudes of a few degrees. Now the fluxion of $p = v\frac{r}{s} + \left(\frac{1}{2(m-t)p} - \frac{ss}{2}\right)\frac{rr}{ss}$, which we may call $p = v\frac{r}{s} + \left(\frac{1}{w} - \frac{ss}{2}\right)\frac{rr}{ss}$, is $dp = \left(\frac{v}{s} + \left(\frac{1}{w} - \frac{ss}{2}\right)\frac{2r}{ss}\right)dr - \frac{rr}{ssw}\left(\frac{dm-dt}{m-t} + \frac{dp}{p}\right)$, the coefficient of dr being equal to $\frac{2p}{r} - \frac{v}{s}$; and $\left(2p - \frac{rv}{s}\right)\frac{dr}{r} = \left(p + \frac{rr}{2ssw}\right)\frac{dp}{p} + \frac{rr}{2ssw}\left(\frac{dm-dt}{m-t}\right)$; $\frac{1}{w}$ being 3.41, and $m - t$, on this supposition, 519. The proportional variation of p, or $\frac{dp}{p}$, will be $\frac{1}{130}$ for every degree that the thermometer varies from 50°; and $\frac{dm}{m}$ being also $\frac{1}{130}$, $\frac{dm}{m-t}$ will be $\frac{766}{519 \times 500} = .003$. The variation of t can only be determined from conjecture; but supposing the alteration of temperature to cease at the height of about 4 miles, it must increase, with every degree that the thermometer rises at the earth's surface, about $\frac{1}{120}$, and $\frac{dt}{t}$ being $\frac{1}{120}$, $\frac{dt}{m-t}$ will be $\frac{247}{519 \times 120} = .004$. The alterations of the barometer will affect p only, $\frac{dp}{p}$ being $\frac{1}{30}$ for every inch above or below 30. It is evident, since $m = \frac{3958 \times 5280 \times 12}{13.57\,bd}$, b being the height of the barometer, and d the bulk of air compared to that of water, that m must diminish, as well as p, when the temperature increases; and the correction for t being subtractive, the three variations will co-operate in their effects; but the proportion will be somewhat different from that of the simple densities. If we preferred the expression derived from Professor Leslie's hypothesis, we should merely have to substitute $\frac{2dn}{1+2n}$ for $\frac{dt}{m-t}$, and the variation depending on the law of temperature would become about $\frac{3}{4}$ as great. It must however be limited to such changes as affect the lower regions of the atmosphere only, its "argument" being the deviation from the mean temperature of the latitude; but even in this form it cannot be satisfactorily applied to the observations at present existing; although it appears to be amply sufficient to explain the irregularities of terrestrial refraction, as well as the uncommon increase of horizontal refraction in very cold countries: and we may even derive from all these considerations a correction of at least half a second, or perhaps of a whole second, for the sun's altitude at the winter solstice, tending to remove the discordance, which has so often been found, in the results of some of the most accurate observations of the obliquity of the ecliptic.

T. Y.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

MDCCCXIX.

METEOROLOGICAL JOURNAL
for January, 1818.

1818	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Jan. 1	8	0	28	41	30,01	26		NW	1	Foggy.
	2	0	33	48	30,02	33		NE	1	Fine.
2	8	0	33	41	30,01	28		NW	1	Cloudy.
	2	0	34	47	29,96	36		E	1	Cloudy, thick weather.
3	8	0	32	48	29,61	31		SE	1	Snow.
	2	0	35	47	29,57	35		SW	1	Cloudy.
4	8	0	41	43	29,48	35		SSW	1,2	Rain.
	2	0	43	45	29,51	45		SSW	1	Cloudy.
5	8	0	42	43	29,52	37		SSW	2,3	Rain.
	2	0	43	48	29,42	45		SW	1,2	Rain.
6	8	0	34	44	29,88	34		NW	1	Cloudy.
	2	0	41	48	30,09	41		WNW	1	Fair.
7	8	0	39	45	30,15	34		SSW	1	Foggy.
	2	0	45	49	30,02	47		SW	1	Cloudy.
8	8	0	38	47	30,06	37		NNW	1	Cloudy.
	2	0	42	52	30,19	43		NW	1	Fine.
9	8	0	39	49	30,16	34		S	1	Fair.
	2	0	49	55	29,95	49		W	1	Rain.
10	8	0	48	50	29,83	46		W	1	Cloudy.
	2	0	50	55	29,81	53		W	1	Rain.
11	8	0	47	51	29,79	48		W	1	Cloudy.
	2	0	50	52	29,72	51		W	1	Cloudy.
12	8	0	40	48	29,84	38		NNW	1	Fair.
	2	0	44	53	30,08	44		WbyS	1	Cloudy.
13	8	0	48	50	29,84	41		W	1,2	Cloudy.
	2	0	50	54	29,80	52		W	2	Cloudy.
14	8	0	47	54	29,84	45		NW	1	Cloudy.
	2	0	45	55	29,87	52		SW	1	Fair.
15	8	0	50	54	29,69	42		W	2,3	Cloudy.
	2	0	53	58	29,67	55		W	1,2	Rain.
16	8	0	47	55	29,88	47		W	1	Cloudy.
	2	0	49	59	29,81	52		W	1	Rain.

Rain this Month 1,461 Inches.

METEOROLOGICAL JOURNAL

for January, 1818.

1818	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Jan. 17	8	0	40	56	29,88	39		NE	1	Fair. { A remarkably fine
	2	0	43	57	29,82	43		NE	1	Fine. { day for the season.
18	8	0	39	53	29,92	37½		WNW	1	Fair.
	2	0	41	54	30,02	43		E	1	Cloudy.
19	8	0	34	49	30,46	32		W	1	Fine.
	2	0	42	57	30,50	42		W	1	Fine.
20	8	0	36	50	30,37	34		WSW	1	Fine.
	2	0	42	57	30,25	43		S	1	Fine.
21	8	0	42	51	29,91	36		W	1	Rain.
	2	0	45	57	30,07	46		WbyN	1	Fine.
22	8	0	39	51	30,00	34		SW	1,2	Fine. { A violent squall
	2	0	44	56	29,80	46		SW	1,2	Cloudy. { of wind and hail
23	8	0	36	51	29,72	34		WSW	1	Fine. { at 10 p. m.
	2	0	41	54	29,59	43		W	1	Cloudy.
24	8	0	40	51	29,51	35		W	1	Rain.
	2	0	42	57	29,51	45		NW	1	Fine.
25	8	0	36	49	29,98	33		NW	1	Hazy, but fine.
	2	0	43	49	30,00	43		W	1	Fair.
26	8	0	47	49	29,89	41		W	1	Cloudy.
	2	0	49	54	29,70	50		W	1	Cloudy.
27	8	0	40	51	29,94	36		W	1	Fine.
	2	0	44	54	29,83	55		SW	1	Cloudy.
28	8	0	41	52	29,58	39		W	1	Cloudy.
	2	0	43	57	29,67	45		W	1	Cloudy.
29	8	0	37	49	29,71	33		W	1	Fair.
	2	0	41	55	29,54	44		S	1	Fine.
30	8	0	41	52	29,02	40		SW	1,2	Rain. { A violent storm of
	2	0	45	57	28,84	46		W	1	Cloudy. { wind from 12-5.
31	8	0	37	51	29,30	35		W	1	Fine.
	2	0	39	57	29,43	39		W	1	Fine.

Rain this Month 1,461 Inches.

METEOROLOGICAL JOURNAL

for February, 1818.

1818	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Feb. 1	8	0	34	49	29.18	34		W	1	Rain and snow.
	2	0	42	52	29.14	42		WNW		Fine.
2	8	0	32	47	28,97	29		SE	1	Hazy.
	2	0	33	53	28,96	33		N	1	Cloudy.
3	8	0	31	47	29.14	27		E	1	Fine.
	2	0	37	53	29.14	39		WNW	1	Rain.
4	8	0	32	46	29,14	28		NE	1	Cloudy.
	2	0	34	49	29,11	35		N	1	Cloudy.
5	8	0	31	45	29.54	28		SW	1	Cloudy.
	2	0	44	53	29,69	44		W	1	Cloudy.
6	8	0	33	49	29.97	31			1	Thick fog.
	2	0	40	54	29.98	38		E	1	Hazy.
7	8	0	31	48	29,08	29			1	Thick fog.
	2	0	31	53	29,09	31		S	1	Cloudy and hazy.
8	8	0	27	45	30,03	26		NbyE	0,1	Cloudy and hazy.
	2	0	30	45	29,99	31		W	1	Hazy.
9	8	0	26	42	30,03	26		W	1	Cloudy and hazy.
	2	0	32	59	30,04	33		S	1	Cloudy and hazy.
10	8	0	28	43	30,01	26		W	1	Thick fog.
	2	0	33	48	30,05	35		NE	1	Cloudy.
11	8	0	33	45	30,18	33		E	1	Cloudy and rather hazy.
	2	0	37	52	30,22	37		SE	1	Hazy.
12	8	0	31	44	30,20	31		W	1	Cloudy.
	2	0	36	51	30,18	36		S	1	Cloudy.
13	8	0	34	46	30,11	33		SE	1	Cloudy.
	2	0	39	54	30,01	41		SE	1	Fine.
14	8	0	33	47	29,88	29		SE	1	Fine.
	2	0	38	47	29,82	39		S	1	Fair.

Rain this Month 0,727 Inches.

METEOROLOGICAL JOURNAL
for February, 1818.

1818	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.	Rain this Month 0,727 Inches.	Winds. Points.	Str.	Weather.
Feb. 15	8	0	33	47	29,90	30		ESE	1	Hazy.
	2	0	40	49	29,91	42		SE	1	Rain.
16	8	0	39	44	29,91	33		SE	1	Cloudy.
	2	0	46	51	29,94	49		S&W	1	Cloudy.
17	8	0	44	49	29,99	42		SE	1	Cloudy.
	2	0	48	61	29,96½	51		S	1	Fine.
18	8	0	47	54	29,95	44		S	1,2	Rain.
	2	0	48	59	29,93	49		SSW	2	Rain.
19	8	0	42	52	30,09	41		W	1	Foggy.
	2	0	50	61	29,89	52		SbyE	1	Rain.
20	8	0	41	57	30,06	34		S	1	Fine.
	2	0	46	62	30,04	49		N	1	Fine.
21	8	0	44	55	29,70	36		S	2	Cloudy.
	2	0	46	57	29,37	47		NW	1	Rain.
22	8	0	40	52	29,29	39		NW	1	Rain. A fall of snow at 5 p.m.
	2	0	40	51	29,05	41		SE	1	Rain. Barom. 29,27 at 11 p.m.
23	8	0	34	47	29,78	32		N	1	Fine.
	2	0	44	57	29,83	44		S	1	Cloudy.
24	8	0	37	49	29,78	34½		S	1,2	Fine.
	2	0	44	57	29,88	46		W	1	Fair.
25	8	0	48	53	29,59	42		W	1,2	Cloudy.
	2	0	51	60	29,58	52		SW	1,2	Fair.
26	8	0	40	52	29,42	39		W	1	Cloudy.
	2	0	42	60	29,70	56		N	1	Cloudy.
27	8	0	38	52	29,56	34		S	1	Rain.
	2	0	48	62	29,35	50		W	1	Fine.
28	8	0	40	53	29,67	37		WSW	1	Cloudy.
	2	0	46	59	29,61	49		SW	1	Cloudy.

METEOROLOGICAL JOURNAL
for March, 1818.

1818	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds. Points.	Str.	Weather.
			o	o	Inches.					
Mar. 1	7	o	39	55	29,69	38		SW	1	Fair.
	2	o	44	59	29,63	51		W	1	Fair.
2	7	o	40	51	29,70	37		W	1	Fair.
	2	o	45	58	29,60	50		W	1	Rain.
3	7	o	41	53	29,78	37		S	1	Fine.
	2	o	49	57	29,68	50		SW	1,2	Cloudy.
4	7	o	37	52	29,39	35		W	1	Fine.
	2	o	43	55	29,28	48		S	1	Cloudy.
5	7	o	43	51	28,88	42		S	2	Cloudy.
	2	o	43	61	29,01	47		WNW	1	Fair.
6	7	o	41	53	29,29	38		SW	1	Fair.
	2	o	45	62	29,41	49		W	1,2	Cloudy.
7	7	o	43	52	29,24	36		S	2,3	Rain.
	2	o	44	57	28,81	50		W	2	Cloudy.
8	7	o	40	51	28,95	39		W	1,2	Fine.
	2	o	47	54	29,11	52		W	2	Fine.
9	7	o	36	48	29,39	34		SW	1	Fair.
	2	o	42	54	29,42	43		WNW	1	Cloudy.
10	7	o	33	49	29,36	31		W	1	Cloudy.
	2	o	38	57	29,34	41		NW	1	Cloudy.
11	7	o	37	49	29,48	33		W	1	Fair.
	2	o	44	57	29,51	44		W	1	Cloudy.
12	7	o	35	49	29,87	34		W	1	Cloudy.
	2	o	45	57	29,92	46		W	1,2	Cloudy.
13	7	o	37	51	29,28	35		NE	1	Cloudy.
	2	o	42	58	29,66	44		N	1	Fine.
14	7	o	35	50	29,85	32		E	1	Fine.
	2	o	44	58	29,88	46		W	1	Cloudy.
15	7	o	41	50	29,54	40		SW	1.2	Cloudy.
	2	o	45	46	29,39	49		SSW	1	Hazy.
16	7	o	40	48	29,47	36		W	1,2	Cloudy.
	2	o	47	58	29,56	48		NW	1	Fine.

Rain this Month 0,986 Inches.

A violent gale between 10 and 12 last night, baro. 28,61 at 11 30 p.m.

METEOROLOGICAL JOURNAL

for March, 1818.

1818	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Mar. 17	7	0	45	51	29,72	40		W	1	Rain.
	2	0	50	59	29,93	51		N	1	Cloudy.
18	7	0	41	53	30,09	40		W	1	Cloudy.
	2	0	51	60	30,05	53		WNW	1	Cloudy.
19	7	0	46	55	30,01	46		W	1	Cloudy.
	2	0	50	60	29,94	53		WSW	1	Cloudy.
20	7	0	48	56	29,69	48		NW	1	Cloudy.
	2	0	51	68	29,86	52		NW	1	Fine.
21	7	0	40	50	29,97	35		W	1	Fair.
	2	0	46	62	29,88	52		W	1,2	Rain and squally.
22	7	0	46	54	29,66	40½		W	1	Rain.
	2	0	49	57	29,68	54		NW	1	Fine, showery.
23	7	0	44	53	29,28	47		NE	1	Rain and squally.
	2	0	47	63	29,53	52		W	1	Fine, showery.
24	7	0	40	53	29,68	38		WSW	1	Cloudy.
	2	0	48	57	29,68	50		WNW	1	Showery.
25	7	0	39	52	29,56	37		W	2	Cloudy.
	2	0	43	58	29,64	49		N	1,2	Cloudy.
26	7	0	42	51	29,57	34		WNW	1	Rain.
	2	0	40	57	29,22	43		NE	1	Rain.
27	7	0	38	51	30,01	34		W	1	Cloudy.
	2	0	44	48	30,17	46		NW	1	Fine.
28	7	0	39	52	30,33	35		S	1	Cloudy.
	2	0	46	58	30,30	50		S	1	Cloudy.
29	7	0	42	51	30,21	38½		SE	1	Fine.
	2	0	46	53	30,14	52		S	1	Fine.
30	7	0	41	49	30,17	37		S	1	Fine.
	2	0	50	58	30,24	55		W	1	Cloudy.
31	7	0	40	52	30,34	38		SE	1	Fine.
	2	0	46	59	30,31	48		E	1	Cloudy.

Rain this Month 0,986 Inches.

METEOROLOGICAL JOURNAL

for April, 1818.

1818	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
April 1	7	0	43	52	30,26	38		E	1	Fine.
	2	0	44	59	30,22	49		NW	1	Fine.
2	7	0	41	52	30,31	39		N	1	Cloudy.
	2	0	45	59	30,33	47		NE	1	Fine.
3	7	0	41	52	30,36	39		E	1	Cloudy.
	2	0	46	61	30,38	48		SE	1	Cloudy.
4	7	0	40	52	30,40	35		S	1	Fine.
	2	0	46	62	30,37	48		SE	1	Fine.
5	7	0	40	52	30,10	33		SSE	1	Fair.
	2	0	43	55	30,07	54		NW	1	Fine.
6	7	0	48	54	29,31	42		SW	1,2	Cloudy, rain in the night.
	2	0	50	57	29,32	52		N	1,2	Cloudy.
7	7	0	40	51	29,72	39		SSE	1	Rain.
	2	0	49	58	29,56	50		SW	1	Rain.
8	7	0	53	56	29,49	50		S	1	Cloudy.
	2	0	49	59	29,45	62		NE	1	Rain.
9	7	0	53	56	29,37	50		S	1	Fair.
	2	0	54	64	29,38	57		W	1	Cloudy.
10	7	0	47	58	29,61	44		S	1	Rain.
	2	0	52	65	29,50	55		S	1	Rain.
11	7	0	46	58	29,27	46		S	1	Fine.
	2	0	44	57	29,45	59		SSW	1	Rain.
12	7	0	41	53	29,93	46		NNW	1	Fine.
	2	0	47	53	30,03	51		SW	1	Fine.
13	7	0	38	51	30,08	35		S by W	1	Cloudy.
	2	0	48	60	29,96	52		SW	1	Fine.
14	7	0	43	52	29,71	40		SE	1	Hazy.
	2	0	51	60	29,72	56		SE	1	Cloudy.
15	7	0	44	54	29,77	38		E	1	Fine, but rather hazy.
	2	0	51	63	29,70	55		SE	1	Fair.

Rain this Month 1,791 Inches.

METEOROLOGICAL JOURNAL
for April, 1818.

1818	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Sir's Therm.		Winds. Points.	Str.	Weather.
Apr. 16	7 0	45	55	29,44	41		SE	1	Fine.
	2 0	54	63	29,54	56		SE	1	Cloudy.
17	7 0	49	57	29,28	46		E	1	Rain.
	2 0	52	62	29,62	54		E	1	Cloudy.
18	7 0	45	55	29,33	44		E	1	Cloudy.
	2 0	51	68	29,45	54		ESE	1	Fine.
19	7 0	43	55	29,71	43		NE	1	Cloudy.
	2 0	48	56	29,76	50		E	1	Cloudy.
20	7 0	40	53	29,82	35		E	1	Fine, rather hazy.
	2 0	48	58	29,81	50		E	1	Cloudy.
21	7 0	44	54	29,71	35		E	1	Cloudy.
	2 0	52	63	29,70	53		SE	1	Cloudy.
22	7 0	49	55	29,71	45		E	1	Cloudy and hazy.
	2 0	53	59	29,63	56		E	1,2	Rain.
23	7 0	48	54	29,56	45		E	1,2	Rain.
	2 0	45	61	29,48	50		E	1	Thick dark foggy weather.
24	7 0	46	58	29,30	44		ESE	1	Rain.
	2 0	51	62	29,33	53		E	1	Cloudy.
25	7 0	50	58	29,13	43		S	1	Rain.
	2 0	64	63	29,19	55		SW	1	Fair.
26	7 0	52	57	29,42	43		NW	1	Cloudy.
	2 0	61	61	29,47	65		SW	1	Fine.
27	7 0	55	59	29,42	53		E	1,2	Cloudy, but fine.
	2 0	60	65	29,57	64		SW	1	Fine.
28	7 0	50	59	29,83	49		S by E	1	Fair.
	2 0	58	63	29,90	62		SW	1	Fair.
29	7 0	50	61	30,00	42		W	1	Fair.
	2 0	59	65	29,95	64		E	1	Fine.
30	7 0	51	59	29,76	45		E	1	Cloudy and hazy.
	2 0	53	64	29,61	54		E	1	Rain.

Rain this Month 1,791 Inches.

METEOROLOGICAL JOURNAL
for May, 1818.

1818	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
May 1	7	0	53	60	29,75	51		N	1	Fine.
	2	0	69	68	29,85	62		W	1	Fine, but showery.
2	7	0	50	60	29,90	46		E	1	Cloudy.
	2	0	61	66	29,81	64		E	1	Cloudy.
3	7	0	53	60	29,55	49		E	1	Rain.
	2	0	60	63	29,47	64		E	1	Fine.
4	7	0	51	60	29,52	49		W	1	Fine.
	2	0	61	64	29,53	60½		W	1	Fine.
5	7	0	52	60	29,49	49		N	1	Fine.
	2	0	60	63	29,43	64		E	1	Cloudy.
6	7	0	52	59	29,35	51		NE	1	Cloudy.
	2	0	57	63	29,34	61		S	1	Cloudy.
7	7	0	51	58	29,34	49½		S	1,2	Cloudy.
	2	0	58	67	29,39½	61		SSE	1	Cloudy.
8	7	0	52	60	29,57	48		S	1	Fair.
	2	0	61	62	29,57	66		SSE	1	Cloudy.
9	7	0	50	57	29,59½	47		SW	1	Fine.
	2	0	57	61	29,63	61		SSE	1	Showery.
10	7	0	50	58	29,86	47		W	1	Fine.
	2	0	59	60	29,87	64		S	1	Fine.
11	7	0	56	58	29,74	48		S	1	Fine.
	2	0	58	61	29,65	66		E	1	Fine.
12	7	0	54	59	29,63	52		W	1	Cloudy.
	2	0	61	60	29,65	65		W	1	Cloudy.
13	7	0	50	58	29,49	48		SE	1	Rain.
	2	0	58	62	29,40	61		W	1,2	Fine, but showery.
14	7	0	48	57	29,41	46		S	2	Rain.
	2	0	55	58	29,42	57		S	2	Squally rainy weather.
15	7	0	52	58	29,52	47		W	1	Cloudy.
	2	0	58	53	29,52	62		N	1	Cloudy.
16	7	0	53	61	29,56	48½		E	1	Fine.
	2	0	61	60	29,59	66		SE	1	Cloudy.

Rain this Month 1,597 Inches.

METEOROLOGICAL JOURNAL
for May, 1818.

1818	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
May 17	7	0	54	58	29,68	52		SW	1	Cloudy.
	2	0	56	58	29,78	57		NNE	1	Rain.
18	7	0	54	58	29,87	48		NNE	1	Fair.
	2	0	64	66	29,89	69		E	1	Fair.
19	7	0	51	59	29,99	48		NE	1	Cloudy.
	2	0	57	60	30,00	58		N	1	Cloudy.
20	7	0	50	58	30,06	44		NE	1	Fine.
	2	0	64	59	30,06	65		E	1	Fair.
21	7	0	51	57	30,18	47		E	1	Cloudy.
	2	0	57	63	30,18	67		E	1	Fine.
22	7	0	50	58	30,23½	41		E	1	Cloudy.
	2	0	57	63	30,24	60		E	1	Fair.
23	7	0	50	57	30,31	45		E	1	Cloudy, rather hazy.
	2	0	57	59	30,31	59		SE	1	Fine.
24	7	0	53	58	30,34	44		E	1	Cloudy.
	2	0	60	64	30,35	62		E	1	Fair.
25	7	0	53	58	30,33	49		E	1	Cloudy.
	2	0	60	64	30,29	64		EbyS	1	Fair.
26	7	0	59	59	30,27	47		N	1	Fair.
	2	0	62	65	30,27	68		E	1	Fair.
27	7	0	53	59	30,34	47		E	1	Cloudy.
	2	0	63	67	30,29	67½		E	1	Fine.
28	7	0	53	58	30,22	44		N	1	Fine.
	2	6	64	66	30,16½	68		NNE	1	Fair.
29	7	0	52	59	30,12	47		SE	1	Cloudy.
	2	0	59	62	30,12	63		SE	1	Cloudy.
30	7	0	51	59	30,14	45½		ESE	1	Cloudy.
	2	0	55	60	30,11	57½		NE	1	Cloudy.
31	7	0	52	58	30,00	45		SW	1	Cloudy.
	2	0	65	64	29,96	71		SE	1	Fair.

Rain this Month 1,597 Inches.

METEOROLOGICAL JOURNAL

for June, 1818.

1818	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
June 1	7	0	62	60	29,98	57		WSW	1	Cloudy.
	2	0	68	63	29,98	74		NNW	1	Cloudy.
2	7	0	64	63	30,02	60		SW	1	Fine.
	2	0	69	67	30,02	76		SW	1	Fine.
3	7	0	62	63	30,08	53		W	1	Fine.
	2	0	72	70	30,09	76		SW	1	Fair.
4	7	0	63	63	30,16	56		SE	1	Hazy and dark.
	2	0	73	73	30,18	77		S	1	Fair.
5	7	0	63	66	30,30	56		E	1	Cloudy.
	2	0	71	73	30,30½	73		E	1	Fair.
6	7	0	63	66	30,34	55		E	1	Fair.
	2	0	70	73	30,31	75		E	1	Fair.
7	7	0	62	66	30,26	51		E	1	Fair.
	2	0	71	71	30,23	74½		E	1	Fair.
8	7	0	64	66	30,26	56		E	1	Fine.
	2	0	69	73	30,28	74		E	1	Fair.
9	7	0	65	67	30,25½	56		E	1	Fine.
	2	0	69	73	30,24	74		E	1	Fair.
10	7	0	65	67	30,23	53½		E	1	Fine.
	2	0	71	74	30,22	78		E	1	Fair.
11	7	0	68	69	30,14½	60		E	1	Fine.
	2	0	74	75	30,11	82		E	1	Fine.
12	7	0	68	69	30,04	57		E	1	Fine.
	2	0	78	76	29,99	82		E	1	Fine. Ther. 70 at 11 p.m.
13	7	0	67½	70	29,94	63		W	1	Fair.
	2	0	75	77	29,92	85		E	1	Cloudy. { Thunder storm at 4 p. m.
14	7	0	66	70	29,94	65		W	1	Cloudy.
	2	0	69	70	29,99	73		N	1	Cloudy.
15	7	0	63	68	30,08	58		SW	1	Cloudy, but fine.
	2	0	71	72	30,03	75		W	1	Fine.
16	7	0	65	68½	29,98½	61		W	1	Fine.
	2	0	71	70	29,92	75		SSW	1	Cloudy.

Rain this Month 0,407 Inches.

METEOROLOGICAL JOURNAL

for June, 1818.

1818	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.	
June 17	7	0	64	67	29,82	54		SW	1	Cloudy.	
	2	0	68	68	29,75	71		S	1,2	Cloudy.	
18	7	0	59	66	29,75	57		E	1	Fine.	
	2	0	68	67	29,74	72		N	1	Fine.	
19	7	0	59	64	29,82	57		W	1	Cloudy.	
	2	0	65	66	29,81	71		S	1,2	Cloudy.	
20	7	0	58	64	29,65	54		SW	1,2	Fine.	
	2	0	62	64	29,75	69		NW	1,2	Rain.	
21	7	0	59	63	30,02	50		SW	1	Fine.	
	2	0	65	64	29,99	70		S	1	Fine.	
22	7	0	59	64	29,83	62		SW	1	Cloudy.	
	2	0	66	66	29,71	70		SW	2	Cloudy.	
23	7	0	57	62	29,82	59		SW	1	Fine.	
	2	0	65	65	29,89	69		N	1	Fine.	
24	7	0	61	63	29,92	56		N	1	Fair.	
	2	0	70	67	30,02	74		W	1	Fine.	
25	7	0	59	62	30,10	58		SW	1	Cloudy.	
	2	0	72	68	30,04	75		NW	1	Fine.	
26	7	0	64	67	30,05½	60		WbyN	1	Cloudy.	
	2	0	70	67	30,07	76		NW	1	Cloudy.	
27	7	0	64	67	29,96	59		SE	1	Fine.	
	2	0	70	74	29,83	79		ESE	1,2	Cloudy, but fine.	
28	7	0	63	74	29,78	58		NW	2	Cloudy.	
	2	0	66	67	29,86	70		NW	1,2	Cloudy.	
29	7	0	60	63	30,16	58		SW	1	Fine.	
	2	0	71	69	30,19	76		NW	1	Cloudy.	
30	7	0	63	66	30,27	60		W	1	Fine.	
	2	0	70	72	30,24	80		W	1	Fine.	

Rain this Month 0,407 Inches.

METEOROLOGICAL JOURNAL

for July, 1818.

1818	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Jan. 1	7	0	64	66	30,16	61		W	1	Fine.
	2	0	73	71	30,10	78		W	1	Cloudy.
2	7	0	63	66	30,08	61		E	1	Fine.
	2	0	68	69	30,12	70		E	1	Cloudy.
3	7	0	61	64	30,22	53		SW	1	Fine.
	2	0	71	70	30,18	75		W	1	Cloudy.
4	7	0	64	67	30,09	64		NW	1	Cloudy.
	2	0	70	72	30,09	74		NW	1	Cloudy.
5	7	0	65	67	30,08	59		N	1	Fine.
	2	0	70	72	30,06½	75		N	1	Fine.
6	7	0	63	68	30,11	60		S	1	Cloudy.
	2	0	69	73	30,09	73		E	1	Fine.
7	7	0	61	67	30,04	55		E	1	Fine.
	2	0	71	72	29,93	83		E	1	Fine.
8	7	0	63	68	29,88	59		W	1	Cloudy, but fine.
	2	0	69	71	29,91	72		NNW	1	Fine.
9	7	0	62	67	30,11	55		N	1	Fine.
	2	0	68	69	30,13	74		SSE	1	Cloudy.
10	7	0	64	67	30,12	54		W	1,2	Cloudy.
	2	0	69	69	30,08	74		S	1	Cloudy.
11	7	0	65	65	30,19	60		NE	1	Cloudy.
	2	0	70	70	29,99	74		W	1	Fine.
12	7	0	63	68	29,93	63		W	1	Rain.
	2	0	70	70	29,81	74		W	1	Cloudy.
13	7	0	65	66	29,80	61		W	1	Cloudy.
	2	0	71	71	29,90	76		W	1	Fine.
14	7	0	65	67	29,98	59		S	1	Fine.
	2	0	73	74	30,23	79		SW	1	Fine.
15	7	0	68	68	30,30	62		E	1	Fine.
	2	0	74	74	30,30	81		W	1	Fine.
16	7	0	68	68	30,26	64		W	1	Fine.
	2	0	80	76	30,23	85½		N	1	Fine.

Rain this Month 0,361 Inches.

METEOROLOGICAL JOURNAL
for July, 1818.

1818	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
July 17	7	0	68	69	30,18	68		E	1	Rain.
	2	0	72	73	30,15	75		E	1	Fine.
18	7	0	67	68	30,69	59		E	1	Cloudy.
	2	0	72	71	30,03	76		S	1	Cloudy.
19	7	0	68	69	29,95	61		S	1	Cloudy.
	2	0	75	76	29,92	81		E	1	Fine.
20	7	0	67	68	29,97½	63		N	1	Cloudy.
	2	0	72	76	29,95	77		NE	1	Fine.
21	7	0	64	68	29,98	62		W	1	Cloudy.
	2	0	72	72	29,97	76		NW	1	Fine.
22	7	0	67	69	30,08	64		W	1	Fine.
	2	0	75	75	30,10	81		SW	1	Cloudy.
23	7	0	69	70	30,13	65		SE	1	Fine.
	2	0	76	79	30,08	82		SE	1	Fair.
24	7	0	75	73	29,88	67		SE	1	Fair.
	2	0	78	81	29,86	89		SW	1	Fine.
25	7	0	68	74	29,84	69		NW	1	Fine.
	2	0	76	79	29,86	81		W	1	Fine.
26	7	0	68	70	29,88	65		E	1	Fine.
	2	0	75	78	29,84	81		SW	1	Fine.
27	7	0	71	70	29,84	69		SE	1	Cloudy, but fine.
	2	0	65	72	29,92	78		SE	1	Fair.
28	7	0	65	67	30,19	70		SW	1	Cloudy, but fine.
	2	0	71	72	30,24	72		W	1	Fair.
29	7	0	69	66	30,25	61		SW	1	Fair.
	2	0	73	70	30,22	71		NW	1	Fair.
30	7	0	69	69	30,16	70		WNW	1	Cloudy, but fine.
	2	0	75	72	30,14	76		W	1	Fine.
31	7	0	66	68	30,14	63		NW	1	Cloudy, but fine.
	2	0	64	66	30,09	70		NW	1	Rain.

Rain this Month 0,361 Inches.

METEOROLOGICAL JOURNAL
for August, 1818.

1818	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds. Points.	Str.	Weather.
			°	°	Inches.					
Aug. 1	7	0	64	66	30,01	62		NW	1	Cloudy.
	2	0	68	69	30,03	73		NW	1	Fine.
2	7	0	67	69	30,14	65		W	1	Cloudy.
	2	0	68	69	30,17	69		NW	1	Fair.
3	7	0	67	65	30,04	66		NW	1	Cloudy.
	2	0	68	65	30,11	69		N	1	Fair.
4	7	0	63	65	30,14	64		N	1	Fair.
	2	0	69	70	30,08	76		NW	1	Fair.
5	7	0	67	66	30,10	60		ESE	1	Fair.
	2	0	76	76	30,11	84		ESE	1	Fine.
6	7	0	71	72	30,07	75		ENE	1	Fine.
	2	0	70	70	30,05	77		E	1	Fine.
7	7	0	68	67	30,07	65		SE	1	Fair.
	2	0	75	76	30,10	85		SW	1	Fine.
8	7	0	66	67	30,11	64		SW	1	Fair.
	2	0	65	67	30,16	74		SE	1	Fair.
9	7	0	61	62	30,10	60		S	1	Fine.
	2	0	62	68	30,17	69		S	1	Fine.
10	7	0	67	69	30,10	66		N	1	Fair.
	2	0	66	67	30,19	73		NE	1	Fair.
11	7	0	67	68	30,01	65		E	1	Rain.
	2	0	66	66	30,02	69		E	1	Cloudy.
12	7	0	65	66	30,07	68		NE	1	Cloudy.
	2	0	65	66	30,12	69		N	1	Cloudy.
13	7	0	63	65	30,15	61		N	1	Cloudy.
	2	0	70	71	30,14	79		SE	1	Fine.
14	7	0	71	72	30,07	69		N	1	Cloudy.
	2	0	69	70	30,14	76		NE	1	Fine.
15	7	0	66	66	30,25	61		E	1	Cloudy.
	2	0	68	68	30,24	73		SW	1	Fine.
16	7	0	66	67	30,17	68		S	1	Cloudy.
	2	0	65	64	30,19	73		SE	1	Fine.

Rain this Month 0,578 Inches.

METEOROLOGICAL JOURNAL
for August, 1818.

1818	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Aug. 17	7	0	63	68	30,22	67		E	1	Fine.
	2	0	69	70	30,24	70		ESE	1	Fine.
18	7	0	67	69	30,08	63		E	1	Fine.
	2	0	65	67	29,94	73		NE	1	Fine.
19	7	0	56	58	29,91	56		NE	1	Cloudy.
	2	0	67	69	30,02	70		NE	1	Fine.
20	7	0	57	60	29,93	60		SE	1	Fair.
	2	0	65	68	30,05	75		SE	1	Fine.
21	7	0	59	61	29,91	59		SSE	1	Fair.
	2	0	67	68	30,02	69		SE	1,2	Fine.
22	7	0	60	61	30,01	58		ENE	1	Fine.
	2	0	65	66	30,12	68		NE	1	Fine.
23	7	0	53	54	30,02	55		N	1	Fair.
	2	0	63	64	30,28	72		NW	1,2	Fine.
24	7	0	59	60	30,10	61		N	1	Fine.
	2	0	62	63	30,18	67		N	1	Fine.
25	7	0	59	63	30,05	60		N	1	Fair.
	2	0	66	68	30,10	69		NE	1	Fair.
26	7	0	58	61	29,98	60		E	1	Fair.
	2	0	62	65	30,03	67		NE	1	Fair.
27	7	0	67	68	30,07	65		E	1	Fine.
	2	0	75	76	30,19	84		SW	1	Fine.
28	7	0	61	58	29,92	60		S	1	Fair.
	2	0	69	72	29,97	75		SW	1	Fine.
29	7	0	63	64	30,00	60		S	1	Fair.
	2	0	77	77	30,07	80		W	1	Fine.
30	7	0	60	59	29,92	57		S	1	Fair.
	2	0	76	76	29,97	81		W	1	Fine.
31	7	0	57	58	29,95	58		SE	1	Fair.
	2	0	70	71	30,02	77		S	1	Fine.

Rain this Month 0,278 Inches.

METEOROLOGICAL JOURNAL

for September, 1818.

1818	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Sep. 1	7	0	62	64	29,68	69		NE	1	Cloudy.
	2	0	64	66	29,75	72		NE	1	Slight showers.
2	7	0	58	59	30,06	60		E	1	Cloudy.
	2	0	65	66	30,02	69		N	1	Fine.
3	7	0	61	64	29,96	63		NE	1	Cloudy.
	2	0	65	67	29,98	70		E	1	Fine.
4	7	0	59	61	29,97	60		SE	1	Cloudy.
	2	0	64	63	29,95	68		E	1	Rain.
5	7	0	61	63	29,97	62		SSE	1	Cloudy.
	2	0	68	67	29,93	66		S	1	Rain.
6	7	0	64	66	29,75	62		SW	1	Cloudy.
	2	0	60	62	99,87	64		SE	1	Fair.
7	7	0	57	60	29,91	57		NE	1	Cloudy.
	2	0	61	62	30,07	68		N	1	Cloudy.
8	7	0	60	62	30,00	61		NW	1	Cloudy.
	2	0	62	64	30,07	68		N	1	Fine.
9	7	0	62	63	29,96	62		N	1	Cloudy, but fine.
	2	0	69	70	30,06	72		ENE	1	Fair.
10	7	0	60	60	29,94	62		N	1	Rain.
	2	0	65	67	29,96	70		NE	1	Fine.
11	7	0	66	66	29,95	61		N	1	Cloudy.
	2	0	67	69	30,07	72		NW	1	Rain.
12	7	0	62	64	29,96	60		NWbN	1	Cloudy, but fine.
	2	0	66	67	29,94	70		N	1	Cloudy, but fine.
13	7	0	60	61	30,21	61		NW	1	Cloudy.
	2	0	65	66	29,97	72		WNW	1	Fair.
14	7	0	68	68	29,93	66		SE	1	Fair.
	2	0	69	70	30,10	77		SE	1	Fine.
15	7	0	67	68	30,01	65		E	1	Rain.
	2	0	67	69	29,96	70		E	1	Showers.

Rain this Month 0,972 Inches.

METEOROLOGICAL JOURNAL
for September, 1818.

1818	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six'p Therm.		Winds. Points.	Winds. Str.	Weather.
Sep. 16	7	0	56	55	29,84	54		NE	1	Fair.
	2	0	69	69	29,77	70		E	1	Rain.
17	7	0	53	54	29,86	56		E	1	Fair.
	2	0	63	63	29.89	65		N	1	Fair.
18	7	0	51	52	30,03	50		SE	1	Fair.
	2	0	63	64	30,05	67		S	1	Rain.
19	7	0	56	55	29,89	55		SSW	1	Fair.
	2	0	63	65	29,93	69		S	1	Rain.
20	7	0	58	60	29,58	56		N	1	Fair.
	2	0	66	66	29,57	68		N	1,2	Rain.
21	7	0	60	61	29,56	60		NE	1	Rain.
	2	0	68	68	29,60	70		NW	1	Rain.
22	7	0	59	60	29,59	61		N	1	Cloudy.
	2	0	63	65	29,68	69		E	1	Cloudy.
23	7	0	59	59	29,68	58		E	1	Cloudy.
	2	0	67	68	29,77	70		E	1	Cloudy but fine
24	7	0	58	59	29,76	57		NE	1	Hazy.
	2	0	66	68	29,81	70		NW	1	Cloudy.
25	7	0	59	60	29,75	56		N	1	Cloudy.
	2	0	65	67	29,55	68		N	1	Cloudy.
26	7	0	57	58	29,57	55		E	1	Fair.
	2	0	68	69	29,66	70		NW	1	Rain.
27	7	0	58	59	29,65	59		N	1	Cloudy.
	2	0	66	66	29,72	68		N	1	Fine.
28	7	0	59	60	29 63	57		SW	1	Fine.
	2	0	63	64	29,77	67		S	1	Fair.
29	7	0	59	61	29,72	59		S	1	Fair.
	2	0	64	66	29,83	69		S	1	Rain.
30	7	0	58	59	29,85	56		SE	1	Fair.
	2	0	63	65	29,96	67		SE	1	Rain.

Rain this Month 0,972 Inches.

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1818	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Sia's Therm.	Rain this Month 1,166 Inches.	Winds. Points.	Winds. Str.	Weather.
Oct. 1	8	0	54	55	29,57	53		E	1	Cloudy.
	2	0	63	64	29,62	68		NE	1	Cloudy.
2	8	0	51	52	29,43	50		ENE	1	Fair.
	2	0	62	62	29,57	65		NE	1	Rain.
3	8	0	54	56	29,47	53		E	1	Fair.
	2	0	63	64	29,67	67		E	1	Fair.
4	8	0	55	56	29,66	55		E	1	Fair.
	2	0	61	62	29,62	65		N	1	Very heavy rain.
5	8	0	59	59	29,46	57		NW	1	Rain.
	2	0	65	64	29,57	68		N	1	Fair, rain in the night.
6	8	0	55	55	29,43	53		N	1	Fair.
	2	0	60	61	29,35	65		NW	1	Heavy rain.
7	8	0	51	53	29,33	50		N	1,2	Cloudy.
	2	0	59	59	29,52	64		N	1	Cloudy. [o'clock, a.m.
8	8	0	53	53	29,42	51		N	1	Cloudy, very foggy at 9
	2	0	60	60	29,67	63		N	1	Cloudy.
9	8	0	49	52	29,88	41		NE	1	Cloudy.
	2	0	58	57	29,83	60		W	1	Fine.
10	8	0	56	58	29,68	53		S	1	Cloudy.
	2	0	58	62	29,53	61		SW	1	Cloudy.
11	8	0	58	60	29,55	58		S	1	Cloudy.
	2	0	61	61	29,54	63		S	1	Rain.
12	8	0	51	60	29,72	52		S	1	Fine.
	2	0	57	68	29,83	61		W	1	Fine.
13	8	0	55	62	29,87	49		SE	1	Cloudy.
	2	0	62	66	29,88	64		S	1	Cloudy.
14	8	0	57	62	29,99	53		ESE	1	Fine.
	2	0	61	69	29,98	67½		S by E	1	Fine.
15	8	0	59	63	29,95	56		ESE	1	Cloudy.
	2	0	64	66	29,94	67		SSE	1	Cloudy.
16	8	0	59	64	29,97	57		SE	1	Cloudy.
	2	0	66	66	30,00	66		NW	1	Cloudy.

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1818	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H. M.	°	°	Inches.			Points.	Str.	
Oct. 17	8 0	56	64	30,08	56		S	1	Foggy.
	2 0	62	65	30,07	65		ESE	1	Cloudy.
18	8 0	55	62	29,94	51		S	1	Cloudy.
	2 0	61	63	29,95	63		S	1	Fine.
19	8 0	54	62	29,98	54		W	1	Foggy thick weather.
	2 0	58	62	29,98	61		SSW	1	Cloudy and dark.
20	8 0	53	61	30,06	53		SE	1	Cloudy.
	2 0	59	64	30,10	61		SE	1	Fine.
21	8 0	48	59	30,19	44		SE	1	Foggy.
	2 0	54	61	30,15	56		S	1	Fine.
22	8 0	49	57	30,00	48		E	1	Fine.
	2 0	54	57	29,98	55		ESE	1	Cloudy, but fine.
23	8 0	49	56	29,97	54		SE	1	Cloudy.
	2 0	52	55	29,98	54		E	1	Cloudy.
24	8 0	48	55	30,05	48		E	1	Cloudy.
	2 0	50	52	30,15	51½		SE	1	Cloudy.
25	8 0	50	54	30,03	48		E	1	Cloudy and hazy.
	2 0	55	59	30,03	58		E	1	Fine.
26	8 0	51	55	30,06	50		E	1	Hazy.
	2 0	59	60	30,06	60		E	1	Fine.
27	8 0	51	56	30,12	50		E	1	Hazy.
	2 0	58	62	30,14	60		E	1	Fine.
28	8 0	54	57	30,15	52		W	1	Fine.
	2 0	61	61	30,16	61		NW	1	Cloudy.
29	8 0	54	58	30,28	55		W	1	Cloudy thick weather.
	2 0	56	58	30,32	60		W	1	Fine.
30	8 0	51	57	30,30	52		W	1	Fine, rather hazy.
	2 0	57	58	30,07	59		W	1	Cloudy.
31	8 0	53	57	30,07	53		SSW	1	Cloudy.
	2 0	56	58	29,95	58		W	1	Rainy.

Rain this Month 1,166 Inches.

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1818	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Nov. 1	8	0	51	56	29.90	51½		SW	1	Hazy and cloudy.
	2	0	55	56	29.95	57		W	1	Cloudy.
2	8	0	50	52	29.94	51½		W	1	Cloudy and hazy.
	2	0	56	57	29.85	59		SW	1	Fine.
3	8	0	53	57	29.77	54		SbyE	1	Fine.
	2	0	55	60	29.71	59		SE	1	Fine.
4	8	0	51	56	29.54	51		SSE	1	Cloudy.
	2	0	53	57	29.44	57		SSE	1,2	Rain.
5	8	0	54	57	29.42	54		ESE	1	Foggy.
	2	0	56	61	29.34	59		SE	1	Cloudy.
6	8	0	54	60	29.34	55		E	1	Cloudy, rain in the night.
	2	0	56	64	29.39	59		SE	1	Cloudy.
7	8	0	52	60	29.65	54		SE	1	Cloudy.
	2	0	55	66	29.76	59		S	1	Fine.
8	8	0	47	58	29.88	46		S	1	Hazy and foggy.
	2	0	52	57	29.88	57		E	1	Cloudy.
9	8	0	46	57	29.95	45		E	1	Thick fog.
	2	0	59	59	29.97	58		SE	1	Hazy.
10	8	0	48	57	29.97	49		E	1	Small drizzly rain and hazy weather.
	2	0	54	56	29.92	54		SE	1	Cloudy.
11	8	0	48	56	29.77	49		E	1	Rain, wind and rain in the night.
	2	0	52	58	29.80	54		E	1	Cloudy.
12	8	0	43	55	29.68	45		E	1	Fine, rather hazy.
	2	0	49	60	29.62	58		E	1	Fine.
13	8	0	50	57	29.60	48		E	1	Fine.
	2	0	55	63	29.61	57		E	1	Fine.
14	8	0	53	59	29.63	53		SE	1,2	Cloudy, rain in the night.
	2	0	56	60	29.59	58		S	2	Cloudy.
15	8	0	50	58	29.53	49		W	1,2	Fine.
	2	0	51	57	29.63	54		W	1	Cloudy.

Rain this Month 1,084 Inches.

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1818	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H. M.	°	°	Inches.			Points.	Str.	
Nov. 16	8 0	51	57	29.64	48		W	1	Rain.
	2 0	57	60	29.54	58		NW	1,2	Cloudy.
17	8 0	47	58	29.75	47		W	1	Fine.
	2 0	51	63	29.86	53		W	1	Fine.
18	8 0	42	56	30.04	42		W	1	Fine.
	2 0	52	62	30.07	50		S	1	Fine.
19	8 0	49	57	30.07	46		S	1	Cloudy, and hazy.
	2 0	52	63	30.06	54		SE	1	Fine.
20	8 0	41	58	29.95	42		E	1	Fine
	2 0	47	59	29.80	48		E	1	Fine.
21	8 0	41	55	29.77	40		SE	2	Cloudy.
	2 0	42	59	29.74	44		E	1	Fine.
22	8 0	39	52	29.75	39		E	1	Cloudy.
	2 0	42	53	29.75	44		E	1	Cloudy, but fine.
23	8 0	49	52	29.55	40		S	1	Rain.
	2 0	54	59	29.61	54		S	1	Fine.
24	8 0	50	57	29.73	49		SW	1	Rain.
	2 0	52	62	29.82	53		WSW	1	Fine.
25	8 0	41	56	30.11	42		W	1	Rain.
	2 0	52	66	30.17	52		S	1	Fine.
26	8 0	51	58	30.14	43		S	1,2	Fine.
	2 0	53	60	30.16	55		SW	1,2	Rain.
27	8 0	52	60	30.48	52		W	1	Cloudy.
	2 0	55	63	30.40	57		W	1	Cloudy.
28	8 0	52	59	30.40	52		SW	1	Fine.
	2 0	56	63	30.44	58		SW	1	Cloudy.
29	8 0	54	59	30.42	54		SW	1	Fine.
	2 0	54	59	30.41	59		SW	1	Cloudy.
30	8 0	51	61	30.23	52		SW	1	Cloudy.
	2 0	53	64	30.21	54		S	1	Cloudy.

Rain this Month 1,084 Inches.

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for December, 1818.

1818	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H. M.	°	°	Inches.			Points.	Str.	
Dec. 1	8 0	45	61	30,05	49		SE	1	Cloudy.
	2 0	45	62	29,97	50		S	1	Cloudy.
2	8 0	46	58	30,00	45		NE	1	Cloudy.
	2 0	45	62	30,01	52		W	1	Cloudy.
3	8 0	47	56	29,65	44		S	2	Rain.
	2 0	48	60	29,61	48		SW	1,2	Cloudy.
4	8 0	46	57	29,53	46		SE	1	Fine.
	2 0	48	58	29,46	51		SE	1	Cloudy.
5	8 0	46	55	29,49	46		SSE	1	Fine.
	2 0	50	60	29,55	52		SW	1	Cloudy.
6	8 0	42	54	29,60	41		SE	1	Fine.
	2 0	49	55	29,61	59		SE	1	Cloudy.
7	8 0	49	55	29,44	46		S	1	Cloudy.
	2 0	50	61	29,48	53		SW	1	Cloudy.
8	8 0	50	56	29,72	49		S	1	Rain.
	2 0	50	61	29,87	54		W	1	Cloudy.
9	8 0	44	56	30,04	44		S	1	Hazy.
	2 0	45	59	30,05	49		S	1	Rain.
10	8 0	40	53	30,14	39		N	1	Fine.
	2 0	45	58	30,15	46		N	1	Fine, rather hazy.
11	8 0	38	54	30,12	37		NE	1	Cloudy.
	2 0	43	56	30,16	43		SW	1	Fine.
12	8 0	36	51	30,14	36		N	1	Cloudy. [weather.
	2 0	43	56	30,14	44		NE	1	Cloudy and foggy, dark
13	8 0	39	52	30,16	38		E	1	Fine.
	2 0	42	52	30,17	43		NE	1	Hazy.
14	8 0	39	51	30,21	38		N	1	Fine.
	2 0	42	58	30,21	43		N	1	Fine.
15	8 0	38	52	30,17	38		N	1	Fine.
	2 0	39	53	30,08	39		NNE	1	Foggy.
16	8 0	32	48	30,02	32		E	1	Fine.
	2 0	34	55	30,08	36		NNE	1	Fine.

Rain this Month 0,722 Inches.

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1818	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Dec. 17	8	0	24	48	30,13	26		W	1	Fine, rather hazy.
	2	0	34	53	30,08	35		NNW	1	Cloudy.
18	8	0	28	46	29,86	28		W	1	Fine, rather hazy.
	2	0	38	54	29,80	40		WbyN	1	Rain.
19	8	0	31	49	30,24	33		W	1	Fine.
	2	0	36	54	30,28	37		W	1	Fine.
20	8	0	43	48	30,13	36		SW	1,2	Cloudy.
	2	0	47	48	30,05	47		NW	1,2	Cloudy.
21	8	0	48	49	30,08	46		W	1	Cloudy.
	2	0	48	49	30,22	48		W	1	Fine.
22	8	0	30	52	30,51	25			1	Thick fog.
	2	0	33	53	30,52	36			1	Thick fog.
23	8	0	32	56	30,43	32		E	1	Thick fog.
	2	0	36	53	30,35	37		E	1	Foggy.
24	8	0	28	48	30,27	30			1	Thick fog.
	2	0	33	53	30,25	35		N	1	Fog.
25	8	0	29	49	30,15	30		W	1	Foggy.
	2	0	37	46	30,08	38		S	1	Hazy and cloudy.
26	8	0	33	45	29,91	35		ESE	1	Cloudy.
	2	0	33	53	29,90	36		SSE	1	Cloudy.
27	8	0	35	44	30,08	35		SW	1	Cloudy.
	2	0	39	45	30,14	40		SE	1	Cloudy.
28	8	0	36	44	30,44	35		N	1	Fine.
	2	0	41	50	30,50	42		N	1	Fine.
29	8	0	35	46	30,61	35		S	1	Cloudy.
	2	0	37	49	30,58	39		N	1	Fine. [white frost.
30	8	0	29	44	30,49	31		NW	1	Cloudy and foggy. A
	2	0	36	51	30,46	37		N	1	Hazy.
31	8	0	33	43	30,43	31		N	1	Thick fog.
	2	0	38	50	30,41	38		N	1	Fine.

Rain this Month 0,866 Inches.

1818.	Thermometer without.			Thermometer within.			Barometer.*			Six's Thermometer.			Rain.†
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	53	28	41.7	59	41	51.4	30.50	29.09	29.82	55	26	41.1	1.461
February	51	26	38.4	62	42	51.6	30.22	29.05	29.72	56	26	37.6	0.727
March	51	33	43.0	68	46	54.5	30.34	28.84	29.67	55	31	43.3	0.986
April	64	40	48.4	68	51	57.9	30.40	29.13	29.71	65	33	48.3	1.791
May	69	48	56.0	68	53	60.4	30.35	29.34	29.84	69	41	55.4	1.597
June	78	57	66.1	77	60	67.8	30.34	29.65	30.03	85	50	66.1	0.407
July	80	61	68.9	81	64	70.4	30.30	29.80	30.05	89	53	69.5	0.361
August	77	53	65.6	77	54	66.4	30.28	29.91	30.08	85	55	68.1	0.278
September	69	51	62.3	70	52	63.5	30.21	29.55	29.86	77	50	64.3	0.972
October	66	48	56.2	69	52	59.6	30.32	29.33	29.87	68	42	56.6	1.166
November	59	39	50.8	66	52	58.7	30.48	29.34	29.85	59	39	51.6	1.084
December	50	24	39.6	62	43	51.7	30.61	29.44	30.07	59	25	40.4	0.806
Whole year			53.1			59.6			29.88			53.5	11.636

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

† The Society's Rain Gage is 114 feet above the same level, and 75 feet 6 inches above the surrounding ground.

Mean variation of the magnetic needle, June 1818, 24° 15′ 45″ West.

Dip about 70° 51′.